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THE

PHYSICAL REVIEW.

THE ELECTRICAL CONDUCTIVITY OF COPPER AS AFFECTED BY THE SURROUNDING MEDIUM.

By Henry S. Carhart.

THE following investigation was undertaken at my suggestion by two students in my laboratory, Mr. Hugh Rodman and Mr. Fred L. Keeler, for the purpose of confirming or refuting the conclusion reached by Professor Fernando Sanford[1] respecting the dependence of the electrical resistance of copper upon the surrounding medium.

The resistance, which Professor Sanford measured under varying conditions, consisted of a copper tube 120 cm. long and 2.5 cm. in diameter, and a copper wire stretched along the axis of the tube. The tube was closed at the ends by copper plates, each provided with a stopcock for emptying and filling the tube. The wire was connected electrically to the plate at one end and passed out through an insulating bushing in the other. The combined resistance of the wire and tube in series was about 0.0335 ohm.

Measurements of this resistance were made by means of a Wheatstone's bridge with proportional coils of 1 and 1000 ohms, and with resistances divided to 0.1. A change of resistance equal to 0.0001 ohm was indicated plainly by a deflection of the

[1] Leland Stanford Junior University Publications. Studies in Electricity, No. 1; Phil. Mag., January, 1893, Vol. 35, No. 212, p. 65.

galvanometer, while a visible deflection could be obtained for a resistance change of one-fifth the above by reversing the current several times so as to cause the effect to be cumulative. Accordingly it would appear that a variation of 0.00002 ohm was the very smallest that the apparatus could indicate.

The tube was filled with various dielectrics, — air, petroleum, wood alcohol, benzine, and the like, — and measurements of the resistance of the wire and tube were made at various temperatures and the results were plotted. Professor Sanford concludes that the dielectric present affects the conductivity of the copper, and that the conductivities in the various liquids are as follows compared with the conductivity in air as unity: —

Petroleum	1.0018
Carbon bisulphide and oil of turpentine	1.0009
Wood alcohol	0.9998
Wood alcohol and benzine	0.9985
Absolute alcohol	0.9981
Wood alcohol and petroleum	0.9973

Professor Sanford also investigated the effect of certain gaseous dielectrics upon the same copper resistance, with the result of finding that the conductivity of copper in alcohol vapor, chloroform vapor, gasoline vapor, and sulphuric ether vapor was lower than in air.

It seemed to me necessary to try the effect of liquid dielectrics only, since a confirmation of Sanford's results with those would establish a strong probability in favor of the others, and *vice versa*.

The resistance apparatus of Messrs. Rodman and Keeler was substantially the same as that of Professor Sanford, except that a well paraffined stopper was used at one end for the wire to pass through. This stopper could be removed for the purpose of cleaning the tube and of examining its interior. The resistance was about 0.0468 ohm at 21°.1 C., or 40 per cent higher than Professor Sanford used. But the method employed for measuring resistance was at least two and one-half times as sensitive, since a change of 0.000008 ohm was very apparent with the galva-

nometer used, a Thomson astatic of about 7000 ohms resistance, made by Elliott Brothers.

The following were the dimensions: —

Length of tube	86.3 cm.
Diameter of tube	2.5 cm.
Diameter of wire	0.7 mm.
Length of wire	90 cm.
Length of tube for thermometer . .	7 cm.
Diameter of tube for thermometer .	1 cm.

No advantage would be gained in using a smaller copper resistance with the method employed, since the sensibility was in no way affected, except that the error arising from ignorance of exact temperature would be larger, not relatively, but in ohms. The difference in resistance between the tube-wire combination and that of a short link of quarter-inch copper rod was measured by means of the Carey Foster method. It is quite certain that no other method exceeds this one in accuracy, if indeed any other equals it. The bridge wire was a Kirchhoff-Wheatstone, wound on a marble cylinder, a well-known form made by Hartmann and Braun. The resistance of this wire is nearly 20 ohms, but it was shunted with a German silver wire of about 0.079 ohm resistance, which was kept in a bath of petroleum so that its temperature could be accurately determined. The bridge wire is divided directly into 1000 parts; reading easily, however, to 10,000 parts. Each division of the bridge wire was equal to about 0.000008 ohm., at 22° C. Whether this was a little more or a little less at 22° C. was immaterial, since only comparative results were aimed at, and not absolute measurements. This value of the resistance of unit length of the bridge wire was obtained, however, by means of two standard ohms, one of them with a Cavendish Laboratory certificate, and having a known difference between them which had been previously determined many times over. Assuming the temperature coefficient of German silver wire to be 0.00044, an allowance was made of 0.00001 ohm in the calculated resistance of the copper wire and tube for each half degree variation from 22° C. If a coefficient of 0.0004 or 0.0005 had been used,

the difference in the result would have been only 2 in the sixth decimal place. The resistance measured is equal to that of 5852 parts of the shunted bridge wire at 21°.1 or to 0.0468 ohm. A change of one degree in the shunt would make a change therefore of $0.0468 \times 0.00044 = 0.00002$ ohm, or 0.00001 ohm per half degree. Any small error in reading this temperature or in the value of the assumed temperature coefficient is practically negligible, since the results are carried to five decimal places only. The bridge wire itself is also German silver. Its temperature was always very nearly the same as that of the shunt. An error of one degree in the temperature of the bridge wire itself entailed an error of only 5 in the eighth decimal place.

The comparison between the copper link referred to and the tube-wire resistance was easily made by means of the auxiliary apparatus attached to the bridge and represented diagrammatically in Fig. 1. S''' is the German silver shunt immersed in petroleum, S'' is the copper link, while the tube and wire are shown at S'. To the end of the tube and the wire were soldered heavy copper wires, and their amalgamated ends dipped into mercury cups, g and h. The link dipped into corresponding cups e and f on the opposite side of the auxiliary apparatus. The several mercury cups shown are of copper, brazed to heavy copper bars arranged as shown. The connections of the link and the resistance to be measured to the ends of the bridge wire are reversed by a quarter turn of the two commutators, so that the links take the position of the dotted lines. Since this commutation reverses the direction of the current through the wire, a reversing

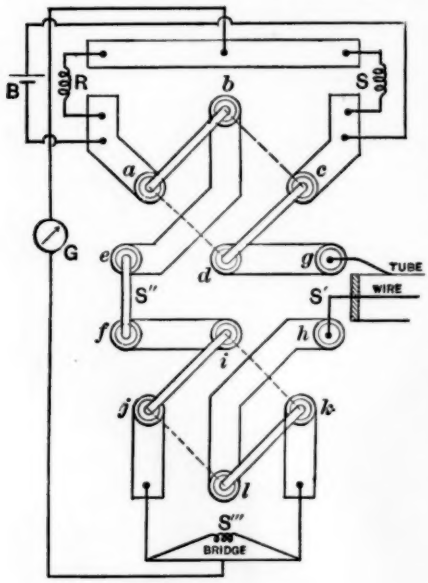


Fig. 1.

key, not shown, was placed in circuit with the battery, in order that the balance could be made for both positions of the tube with respect to the bridge with the current through it in the same direction. The auxiliary resistances R and S, of one ohm each, were wound together in the same box, and their relative resistances were assumed not to change during the time of a single comparison, which required about thirty seconds. It is not probable that any change whatever in their relative resistance took place, since they were made of wire cut from the same spool.

The temperature of the copper resistance was made to vary by means of a hot-water bath, but in no case was the tube allowed to come in contact with the water. Means were employed to make the temperature of all parts of the tube as nearly alike as possible. A thermometer graduated to half degrees was placed with its bulb in the tube next to the wire.

With these appliances a series of measurements were made with the tube filled first with kerosene, then alcohol, then air, and the results were plotted with temperatures and resistances as co-ordinates. The three lines were straight and practically coincident. The tube was then thoroughly cleaned, and after a week the experiments were repeated in the order, air, alcohol, kerosene, as the dielectrics. Again the results were coincident lines. The measurements were repeated several times, using the same dielectrics in various orders, also a mixture of alcohol and kerosene, without once detecting any difference too large to be accounted for by errors of observation.

The Tables I. to VI. give the data for the measurements with the tube containing air, alcohol, kerosene, respectively. a_1 and a_2 are the bridge readings in the two positions of the commutator, b the temperature of the bridge wire and shunt, R the computed resistance of the wire-tube combination, and t its temperature. Two tables belong to each dielectric; one made with the current flowing in one direction through the tube wire, and the other with the current reversed. The circles in the corresponding diagrams show the points obtained with one series of determinations, and the crosses the other. There is no difference between them, showing that there was no thermo-electromotive force present to introduce

TABLE I. — DIELECTRIC AIR.

a_1	a_2	$a_1 - a_2$	b	R	t
7973	1988	5985	20°	0.04784	27°.6
67	90	77	"	78	.3
58	99	59	"	63	26°.4
54	2000	54	"	59	.25
53	02	51	"	57	.0
51	04	47	"	54	25°.9
43	10	33	"	42	24°.8
42	12	30	"	40	.2
40	15	25	20°.5	36	.1
35	20	15	"	29	23°.9
34	20	14	"	28	.65
32	22	10	"	25	.2
29	26	03	"	19	.0
27	28	5899	21°	16	22°.7
24	30	94	"	13	.3
19	35	84	"	05	.0
16	40	76	"	0.04699	21°.8
13	41	72	"	96	.6
10	44	66	"	91	.3
03	51	52	"	80	.1

TABLE II. — DIELECTRIC AIR.

a_1	a_2	$a_1 - a_2$	b	R	t
7975	1985	5990	20°	0.04788	28°.0
70	90	80	"	80	27°.25
80	99	59	"	63	26°.4
54	2001	53	"	58	.2
53	02	51	"	57	.0
51	05	46	"	53	25°.9
43	11	32	"	42	.1
42	13	29	"	39	.0
40	15	25	"	36	24°.8
35	20	15	20°.5	29	.2
33	21	12	"	27	.0
32	22	10	"	25	23°.9
29	28	01	"	18	.45
27	30	5897	"	15	.15
24	31	93	"	12	.0
19	37	82	21°	04	22°.65
16	40	76	"	0.04699	.2
13	41	72	"	96	.0
10	44	66	"	91	21°.8
03	50	53	"	80	.2

TABLE III. — DIELECTRIC ALCOHOL.

a_1	a_2	$a_1 - a_2$	δ	R	t
7988	1967	6021	21°	0.04815	29°.5
79	77	09	"	05	28°.75
76	78	5998	"	0.04796	.3
70	83	87	"	86	27°.7
62	92	70	"	74	.0
58	96	62	"	68	26°.7
56	2000	56	"	63	.35
53	01	52	"	60	.2
49	07	42	"	52	25°.85
47	11	36	"	47	.2
41	15	26	"	39	.0
39	17	22	"	36	24°.8
34	21	13	"	30	.35
31	22	09	"	25	.0
27	28	5899	"	17	23°.7
25	31	95	"	14	.3
23	32	91	"	11	.0
06	46	60	"	0.04686	21°.5

TABLE IV. — DIELECTRIC ALCOHOL.

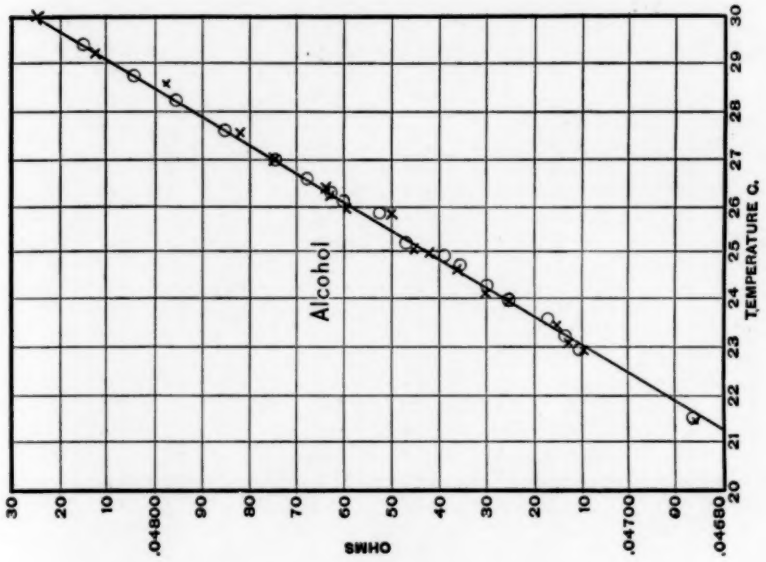
a_1	a_2	$a_1 - a_2$	δ	R	t
7996	1962	6034	21°	0.04825	30°.0
88	69	19	"	13	29°.3
78	78	6000	"	0.04798	28°.6
76	79	5997	"	96	.2
69	89	80	"	82	27°.6
62	92	70	"	74	.0
58	2000	58	"	64	26°.5
56	2000	56	"	63	.3
52	03	49	"	59	.0
48	08	40	"	50	25°.8
46	12	34	"	45	.15
41	13	28	"	41	.0
39	18	21	"	35	24°.75
35	20	15	"	30	.25
31	23	08	"	24	.0
26	30	5896	"	15	23°.55
24	31	93	"	13	.2
22	33	89	"	09	.0
06	47	59	"	0.04685	21°.5

TABLE V. — DIELECTRIC KEROSENE.

a_1	a_2	a_1-a_2	b	R	t
7996	1958	6028	21°	0.04820	30°.0
87	67	20	"	14	29°.35
83	70	13	"	08	.3
76	78	5998	"	0.04796	28°.3
72	83	89	"	89	27°.8
65	93	72	"	76	.0
57	97	60	"	66	26°.45
52	2003	49	"	57	.0
50	05	45	"	54	25°.5
46	11	35	"	46	.2
42	15	27	"	40	24°.7
34	21	13	"	30	.3
25	20	5895	"	14	23°.2
19	36	83	"	06	22°.7
15	40	75	"	0.04698	.2
02	49	53	"	82	21°.25

TABLE VI. — DIELECTRIC KEROSENE.

a_1	a_2	a_1-a_2	b	R	t
7992	1963	6029	21°	0.04821	29°.8
87	70	17	"	12	.15
80	77	03	"	0.04800	28°.6
76	81	5995	"	0.04794	.2
70	89	81	"	83	27°.7
62	94	68	"	72	26°.9
56	99	57	"	64	.0
52	2006	46	"	55	25°.7
48	10	38	"	48	.2
42	13	29	"	41	.0
39	18	21	"	35	24°.5
31	23	08	"	24	.0
22	35	5887	"	08	23°.0
17	39	79	"	0.04700	22°.4
14	43	71	"	0.04695	.0
02	53	59	"	82	21°.2


Alcohol
OHMS
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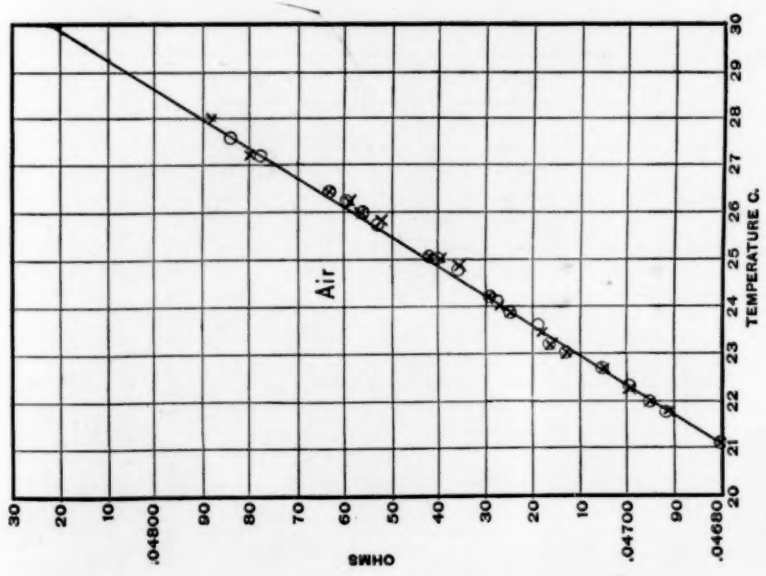
.04700
.04680
TEMPERATURE C.

Fig. 3.


Air
OHMS
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.04700
.04680
TEMPERATURE C.

Fig. 2.

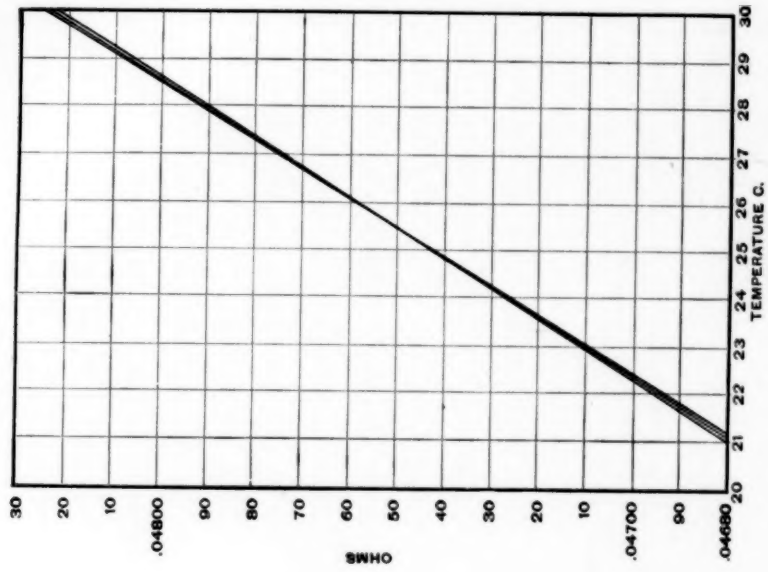


Fig. 5.

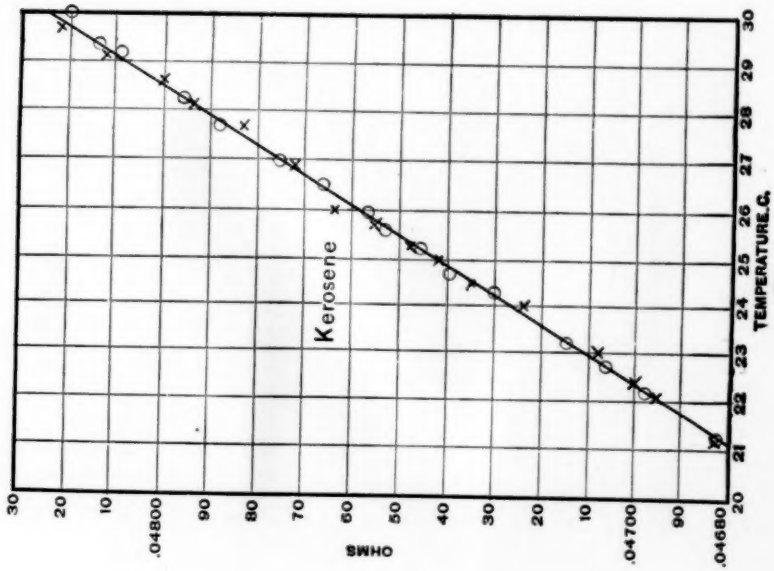


Fig. 4.

an error. Figs. 2, 3, and 4 were plotted directly from the tables, and Fig. 5 shows the three curves transferred to one sheet. It is hardly too much to say that a single line would represent the entire six sets of observations. Certainly there is no difference here which may not properly be ascribed to errors of observation, and there are no such parallel lines at a sensible distance, as Professor Sanford found. Fig. 6 has been plotted from his observations

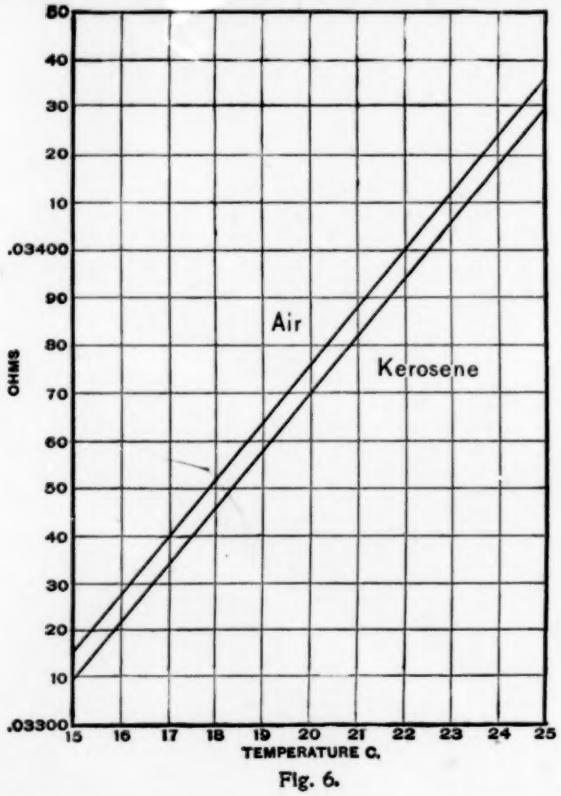


Fig. 6.

with air and kerosene. The vertical distance apart of the two lines is six or seven parts in the fifth decimal place. This is equivalent to eight parts on the bridge wire in our investigation, a difference which would have made a large deflection on our galvanometer. Professor Sanford's air-line lies above that of kerosene. Ours cross at a very minute angle, and if they were forced into parallelism, the air-line would fall below that of kerosene.

TABLE VII.

DIELECTRIC AIR.

a_1	a_2	a_1-a_2	b	R	t
8188	1724	6464	21°.5	0.05169	29°.4
86	28	58	"	64	.2
83	29	54	"	61	.0
82	31	51	"	59	28°.85
78	33	45	"	55	.55
75	37	38	"	48	.4
73	40	33	"	45	.05
70	43	27	"	41	27°.9
64	48	16	22°.0	33	.55
61	50	11	"	29	.4
60	54	06	"	25	.1
58	56	02	"	22	26°.9
52	58	6394	"	15	.6
50	62	88	"	10	.4
47	63	84	"	07	.2
46	66	80	"	04	.0
42	68	74	"	0.05099	25°.8
40	71	69	"	95	.5
38	73	65	"	92	.4
35	77	58	22°.5	87	.0
30	81	49	"	80	24°.7
27	84	43	"	75	.5
25	88	39	"	72	.4
23	89	34	"	68	.1
16	93	23	"	59	23°.7
12	97	15	"	53	.4
06	1802	04	"	44	22°.8
05	06	6299	23°.0	41	.7
02	08	94	"	37	.5
8099	11	88	"	32	.3
96	13	83	"	28	.1
94	15	79	"	25	21°.9
93	18	75	"	22	.6

TABLE VIII.

DIELECTRIC KEROSENE.

a_1	a_2	$a_1 - a_2$	b	R	t
8185	1724	6461	21°.5	0.05168	29°.95
85	28	57	"	65	.4
84	30	54	"	62	.0
74	35	39	"	50	28°.55
74	37	37	"	49	.4
73	39	34	"	46	.2
65	41	24	"	38	27°.85
60	48	12	"	29	.55
60	52	08	"	25	.3
59	54	05	"	22	.0
52	55	6397	"	17	26°.6
51	62	89	"	10	.4
48	67	81	"	04	.1
46	69	77	"	01	25°.9
39	73	66	"	0.05092	.4
33	76	57	22°	86	.05
33	84	49	"	79	24°.7
32	87	45	"	76	.4
20	89	31	"	65	.0
15	99	16	"	53	23°.5
11	1800	11	"	49	.4
11	02	09	"	47	.0
08	02	06	"	45	22°.9
02	04	6298	"	38	.8
01	11	89	21°.5	32	.4
8098	15	83	"	27	.15
97	19	78	"	23	21°.9

TABLE IX.

DIELECTRIC ALCOHOL.

a_1	a_2	a_1-a_2	δ	K	t
8184	1724	6460	22°.5	0.05170	29°.4
82	28	54	"	64	.2
79	31	48	"	59	28°.9
77	37	40	"	53	.55
72	38	34	"	48	.35
70	42	28	"	43	.15
68	43	25	"	41	27°.9
66	46	20	"	37	.7
63	49	14	"	32	.4
60	54	06	"	26	.2
58	58	00	"	21	26°.95
53	62	6391	22°	13	.5
46	65	81	"	05	.2
45	66	79	"	03	.1
42	68	74	"	0.05099	25°.9
40	71	69	"	95	.75
38	73	65	"	92	.5
36	77	59	"	87	.35
33	79	54	"	83	.0
29	79	50	"	80	24°.7
24	79	45	"	76	.5
22	90	32	"	66	.2
17	88	29	"	63	23°.9
16	89	27	"	62	.7
15	1800	15	"	52	.4
11	03	08	"	46	.2
05	07	6298	"	38	22°.8
03	09	94	"	35	.5
8099	12	87	"	30	.1
97	15	82	"	25	21°.9
95	19	76	"	21	.7

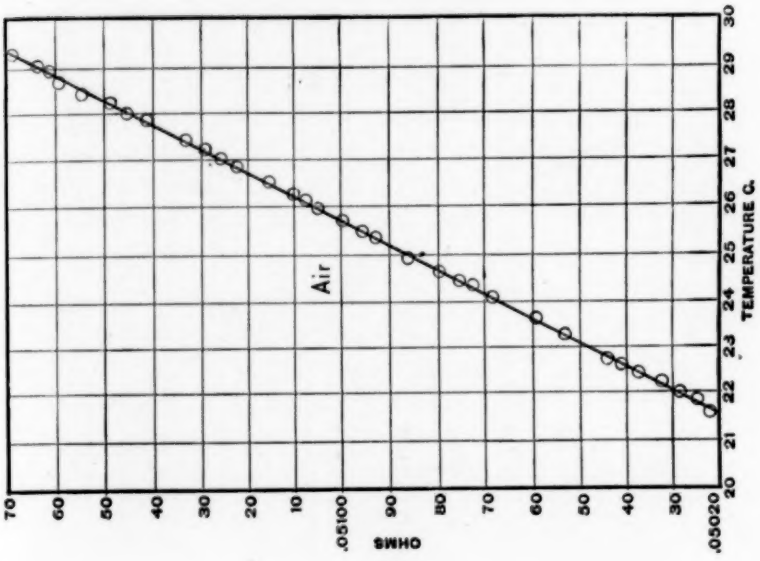
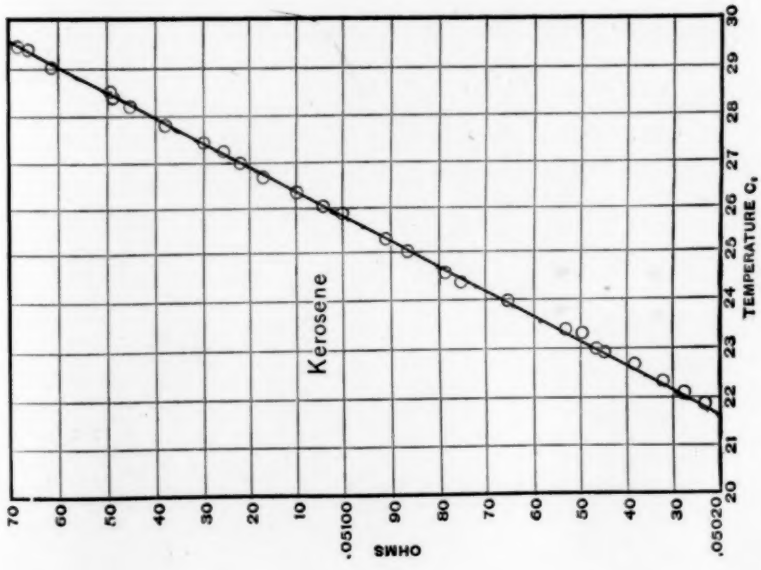

Air
OHMS
.05100
.05020
TEMPERATURE C.

Fig. 7.


Kerosene
OHMS
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.05020
TEMPERATURE C.

Fig. 8.

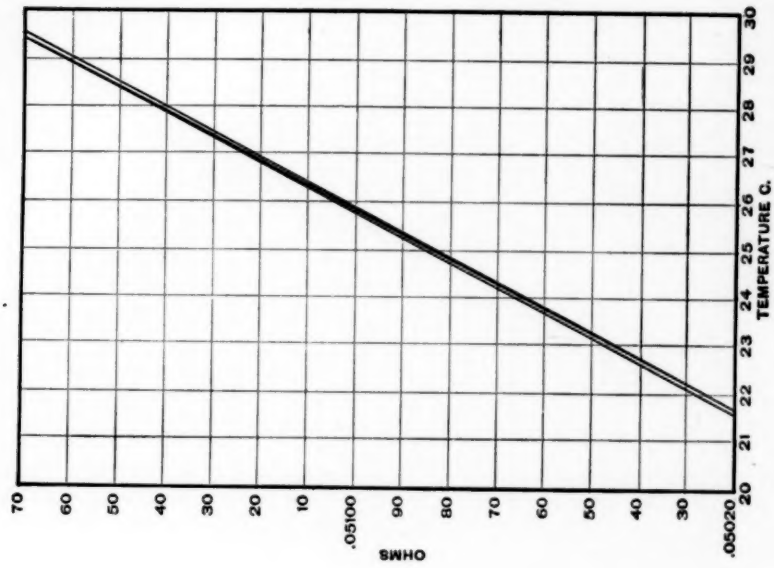
70 60 50 40 30 20 10 .05100 90 80 70 60 50 40 30 .05020
OHMS
20 21 22 23 24 25 26 27 28 29 30
TEMPERATURE C.

Fig. 10.

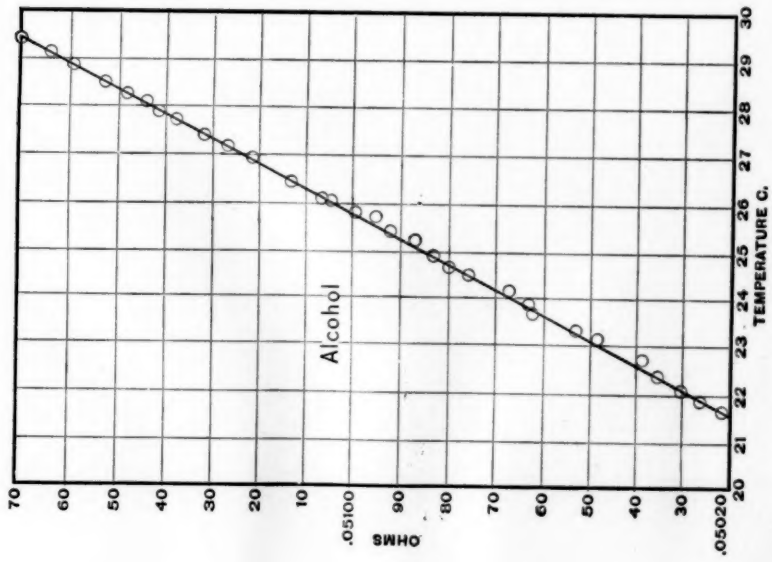
Alcohol
70 60 50 40 30 20 10 .05100 90 80 70 60 50 40 30 .05020
OHMS
20 21 22 23 24 25 26 27 28 29 30
TEMPERATURE C.

Fig. 9.

Such series of observations as the above were taken a number of times, using the dielectrics air, alcohol, kerosene, in various orders; also a mixture of alcohol and kerosene as already stated; but in no case was a difference in resistance detected which was due to the presence of the dielectric.

The procedure was also varied by measuring the resistance of a helix of copper wire immersed in air, alcohol, and kerosene, in succession. The observations are contained in Tables VII., VIII., IX., and are plotted in the curves of Figs. 7, 8, and 9. The three are drawn for comparison in Fig. 10. Any such difference as Professor Sanford found would have been detected in our measurements with this resistance as readily as if it had been as small as his. Doubling the current made no difference in the result. The heating effect of keeping the key closed for twenty or thirty seconds was tried, and in no case did the resistance increase more than 0.00001 ohm.

Professor Sanford's lines are distinctly apart; ours are practically coincident. On the supposition that his are correct, it would be a strange error indeed that would in all cases bring ours together, especially when our method has the great advantage of increased sensibility. We can only conclude that his measurements are affected by some systematic error which he has not discovered.

Mr. Rodman and Mr. Keeler have devoted a good deal of time to this investigation, and have made all the observations. I am indebted to them not only for the tables, but also for the drawings from which the curves have been made.

THE USE OF THE ROTATING SECTORED DISC IN PHOTOMETRY.

ERVIN S. FERRY.

FROM the fact that in photometric measurements the minimum error of observation is introduced when the photometer reading is at the middle of the bar, and the error rapidly increases as the reading is displaced toward one end, the comparison of two lights of different luminosities is attended with considerable uncertainty. The most obvious plan to bring the photometer reading into the region of least error is to introduce into the path of the more intense ray an absorbing medium whose coefficient is constant for all wave lengths. A pair of Nicol's prisms is sometimes employed for the same purpose, and the law of Malus used to compute the percentage of light transmitted; but the ordinary method is to use a rapidly rotating disc from which radial sectors have been removed. The assumption here made is that the ratio of the amount of light transmitted to the total incident illumination, is equal to the ratio of the aperture of the disc to the entire disc. Several experimenters have observed, however, that the candle power of an arc light as obtained by the use of the rotating sectored disc is sometimes considerably less than the value obtained without it. It was in the study of this anomaly that the following experiments were undertaken.

The apparatus consisted of an ordinary 1000 division photometer bar, having near one end the sectored disc rotated by a small electric motor. The disc was on a hinged shelf arranged so that it could be quickly moved out of the path of the ray of light. Various photometers were used according to their especial fitness for the particular conditions of the experiment. The Bunsen was found best for the comparison of lights of considerable difference in color; the Lummer-Brodhun was used when the two lights were of the same quality, while a modification of

Ritchie's photometer gave the best results where the two lights differed slightly in quality. The Nichols horizontal slit spectrophotometer was used in the comparison of monochromatic lights.

As the Nichols-Ritchie photometer has not yet been described, it may not be out of place to here give a brief description of it. The principal object in its construction was to have the two luminous spots one above the other so as to eliminate any error due to a difference between two eyes. It consists of two vertical mirrors (Fig. 1), one above the other, at right angles to each other, and each at 45° to the direction of the photometer bar. These mirrors are placed in a box having open ends, and a circular aperture in the front side covered with a thin sheet of celluloid or other translucent material. From this arrangement, it is evident that the upper half of the translucent screen will be illuminated by the light at one end of the bar, while the lower half will receive its illumination from the other end. The reading consists in moving the photometer along the bar till the line separating the two halves of the disc becomes invisible. By exercising proper care in fitting the mirrors together and in selecting the translucent screen, this photometer can be made as sensitive to differences of color and luminosity as the Lummer-Brodhun, and as convenient to use as the Bunsen.

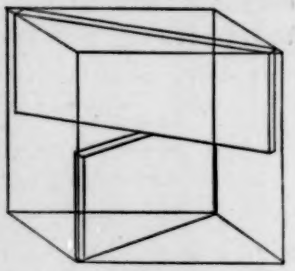

Fig. 1.

The first experiment made was to determine if the error of the disc depends upon the size or arrangement of the separate apertures. For this purpose, discs were made containing 12, 8, 4, 2, and 1 equal and equally spaced segments; also others having the segments in each disc of different sizes, and arranged unsymmetrically. All of these discs were double, so that quite a range of total apertures could be obtained with each pair. When they were revolved with sufficient rapidity to produce a steady illumination of the photometer screen, it was found that the error of the disc was constant for all arrangements of the segments, so long as the total aperture was the same.

From the preceding experiment, the error appeared to be a function of the total aperture only.

The relation between the error and the total aperture was determined as follows: A lime light was placed at the disc end of the photometer bar, and a 16 C.P. incandescent lamp was used as a standard at the other end. The double disc, containing twelve fifteen-degree sectors, was used in this and the succeeding experiments. This disc had a graduated scale on its edge, so that the aperture of each segment could be varied from zero to fifteen degrees. The method of observation was as follows: a photometer-reading was taken with the rapidly rotating disc in the

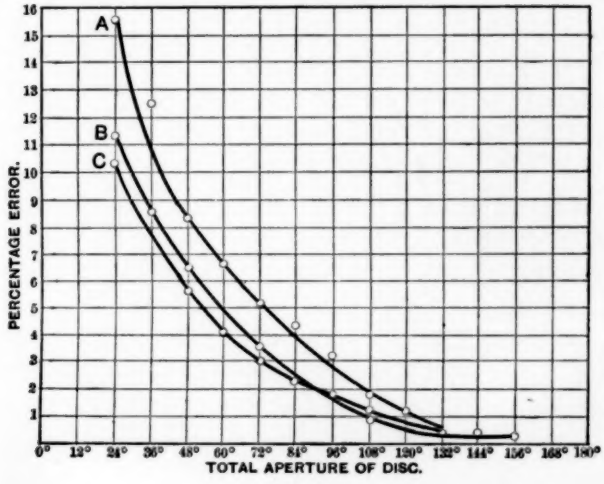

Fig. 2.

path of the stronger ray; the shelf containing the disc was then quickly raised out of the path of the ray, and another reading taken. The aperture of the disc was then changed, and another pair of readings taken. Denoting by I, the light ratio when the disc is in the path of the stronger ray; I_0, the light ratio without the disc; a, the angular aperture of the disc; and $a+b$, the whole circumference of the disc, we have the equation.

$$I=\frac{a}{a+b}\,I_0.$$

The relation between I obtained in this way and by direct observation will then give the error introduced by the disc. The observations and deductions thus obtained are given in the following table, while the results are graphically shown in Curve C, Fig. 2.

TABLE I.

COMPARISON OF LIME LIGHT WITH 16 C.P. INCANDESCENT LAMP.

Observations.			Deductions.			
Aperture.	With disc.	Without disc.	I_0.	I calculated.	I observed.	Per cent error.
24°	546	227	11.596	0.77	0.69	10.4
36°	493	227	11.596	1.16	1.06	8.6
48°	453	227	11.596	1.55	1.46	5.8
60°	424	227	11.596	1.93	1.85	4.1
72°	403	229	11.335	2.27	2.20	3.0
84°	385	230	11.208	2.61	2.55	2.3
96°	371	232	10.958	2.93	2.87	1.7
108°	358	223	10.836	3.25	3.21	1.2
120°	347	234	10.716	3.57	3.54	0.8
132°	339.5	237	10.365	3.80	3.79	0.2
144°	345	250	9.000	3.60	3.60	0
156°	342	255	8.536	3.69	3.70	0
168°	335	256	8.446	3.94	3.94	0
180°	328.5	257	8.358	4.18	4.18	0

Several attempts were made to obtain a similar curve for the case of the arc light; but these attempts were all unsuccessful with ordinary arc lamps, on account of their unsteadiness. But finally, through the courtesy of Mr. L. B. Marks, an "incandescent-arc" lamp[1] was obtained, which met the required condition of steadiness excellently. Table II. and Curve A, Fig. 2, give the disc's error in the case of the arc light.

[1] For description, see Electrical World, Sept. 9, 1893.

TABLE II.

COMPARISON OF ARC LAMP (65 VOLTS AND 8.2 AMPERES) WITH 16 C.P. INCANDESCENT LAMP AT 110 VOLTS.

Observations.			Deductions.			
Aperture.	With disc.	Without disc.	I_0.	I calculated.	I observed.	Per cent error.
24°	521	205	15.039	1.002	0.846	15.6
36°	458	200	16.000	1.600	1.400	12.5
48°	410	195	17.042	1.136	2.071	8.2
60°	373	190	18.175	3.029	2.826	6.7
72°	350	190	18.175	3.635	3.449	5.1
84°	336	193	17.484	4.079	3.905	4.2
96°	320	193	17.484	4.662	4.516	3.1
108°	318	202	15.606	4.682	4.600	1.7
120°	297	195	17.042	5.681	5.603	1.2
132°	290	198	16.407	6.016	5.994	0.3
144°	279	197	16.615	6.646	6.678	0
156°	261	188	18.655	8.083	8.017	0
168°	256	190	18.175	8.481	8.446	0
180°	249	190	18.175	9.088	9.097	0

When a 16 C.P. incandescent lamp was compared with a standard Methven slit, Curve B, Fig. 2, was obtained.

Two 16 C.P. incandescent lamps were then compared. Two were selected that were as nearly alike in every respect as possible, and connected in multiple to a storage battery circuit. Their spectra were examined photometrically, color for color, and found to be nearly alike in luminosity throughout. They were then mounted on the photometer bar, and light ratios obtained with and without the sectored disc. The electromotive force of the lamps was varied from 70 to 120 volts, and the aperture of the disc varied throughout its range, without any appreciable difference being observed between the calculated and observed value of the light that passed through the sectored disc. This experiment shows that the error introduced by the disc is not a function of the absolute illumination of the photometer screen.

Two incandescent lamps giving nominally 150 C.P. and 16 C.P., respectively, were then mounted on the photometer bar. They

were connected to separate circuits, and the included resistances varied till they were of the same color. Again, little or no error was observed, even when the disc was nearly closed, but if the lamp at the disc end of the bar was made to burn at a voltage much greater than normal, the error again appeared. This showed that the error of the disc is not dependent upon the luminosity ratio of the two lights and suggested the possibility that the color of the light might be the cause of the discrepancy. It is to be noticed that in the above four cases where a marked error occurred, the light at the disc end of the bar was of a bluer quality than the standard.

If, after the two lights have been brought to the same color, a sheet of glass slightly tinted with blue be interposed in the path of the ray from the disc end of the bar, error values are obtained similar to those shown in Fig. 2. This seems to prove that the error of the disc is dependent upon the color of the light that it interrupts.

The phenomena described in these experiments can probably be explained from considerations of retinal inertia. The luminosity of ordinary light is the integral of the luminosity of all the wave lengths that affect the retina. The distribution of luminosity in the spectra of ordinary illuminants proceeds from a maximum in the orange or yellow, to zero at the ends of the spectrum. Both the intensity of the retinal sensation[1] and the time light must act upon the retina in order that it may be seen[2] are direct functions of the luminosity. Hence, if the light acts upon the retina for but a very short time, the elements of low luminosity — *i.e.* at the ends of the spectrum — will not produce their maximum impression. Therefore the value of the candle power deduced from the observation with the disc will be less than the total luminosity of the light. If the curve of luminosity with respect to wave length of any light be known, and also the curve of time necessary to receive a retinal impression with respect to the same range of luminosity, the error of the disc can be prophesied for any aperture.

[1] Fechner, Revision der Hauptpunkte der Psychophysik, p. 184.

[2] J. M. Cattell, The Inertia of the Eye and Brain, "Brain," Part XXXI.

After the disc has attained a speed sufficient to cause an unflickering illumination of the screen, any increase of speed produces no difference in the photometer reading. But it was noticed that if the disc did not revolve rapidly enough to produce a perfectly steady illumination of the photometer screen, *more* light appeared to go through the sectored disc than theoretically should. This can possibly be explained, as has been an analogous phenomenon by Ernst Brücke[1] from the consideration of a building up of the separate impressions upon the retina.

Conclusions.

I. While it is physically true that the proportion of light transmitted by a rapidly rotating sectored disc to the total incident illumination is equal to the ratio of the total aperture of the disc to the entire disc, yet the effect of this light upon the retina will not always be proportional to the ratio of the total aperture of the disc to the entire disc.

II. With mixed light containing elements of different luminosity shining upon the retina, a rotating sectored disc will appear to not cut off all the elements in equal proportion, but will intercept most strongly the elements of low luminosity.

III. With any given light, the error introduced by the use of the rotating sectored disc increases as the aperture of the disc diminishes.

IV. With ordinary illuminants, the error is negligible when the total aperture of the disc is more than one-half the entire disc, but rapidly increases as this aperture is diminished.

NOTE UPON THE ACTION OF INTERMITTENT LIGHT UPON THE RETINA.

From the study of rotating cardboard discs painted black and white in sectors, von Helmholtz deduced the law "if a point of the retina is excited by a light which undergoes regular and periodic

[1] "Ueber den Nutzeffect intermitterenden Netzhautreizungen," Sitzungsberichte, Wiener Akademie, 1864, Vol. 49, II. p. 128.

variations, and which has the duration of its period sufficiently short, it produces a continuous impression, equal to that produced if the light emitted during each period were distributed uniformly throughout the duration of the period."[1] Plateau, Fick, and Dove have repeated the experiments of von Helmholtz and confirmed this law; and yet it was thought useful to test the law by using monochromatic light throughout a wide range of luminosity and wave length.

The method employed was to compare photometrically, color for color, two spectra seen side by side in the eye-piece of a spectrometer, one spectrum being produced by a ray coming directly from the source of light, and the other by a ray from the same source, but made rapidly intermittent by a revolving sectored disc. Sunlight, daylight, and the incandescent electric were used to produce the spectra. A very accurate double slit with micrometer adjustments at the object end of the collimator gave the means of varying the luminosity of the two spectra. The method of transmitting the two rays into the collimator is shown in the figure. Observations were taken throughout the entire spectrum and of luminosities varying 1000 per cent. A difference of luminosity of 2 per cent could have been detected by this method, but no deviation from the law of von Helmholtz was observed.

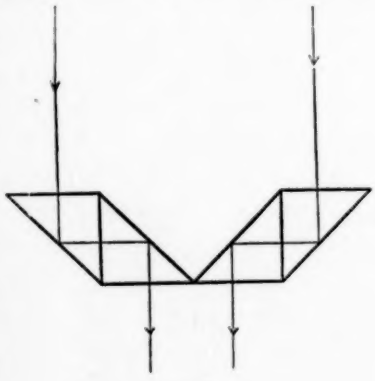

Fig. 3.

[1] Physiologische Optik, 2d ed., p. 483.

FATIGUE IN THE ELASTICITY OF STRETCHING.

BY JOSEPH O. THOMPSON.

ALTHOUGH twenty-eight years have passed since the publication of Lord Kelvin's article, in which he describes the phenomenon of fatigue in the elasticity of torsion, no one to my knowledge has yet demonstrated the existence of a similar fatigue in the elasticity of stretching. In some of Lord Kelvin's experiments, it will be remembered, this phenomenon was very marked. In one experiment[1] he used two exactly similar wires stretched by vibrators of equal weight and equal moment of inertia. No. 1 had been kept at rest for nine days, while No. 2 had been kept oscillating more or less every day during that time. In an experiment then made, No. 1 subsided from an amplitude of 20 scale-divisions to 10, after 100 vibrations in 242 seconds, making a period of 2.42 seconds. No. 2 subsided through an equal angle, after 44 or 45 vibrations, and the period was 2.495 seconds. The increase in the period is thus a shade over 3 per cent, and this means a diminution of over 6 per cent in the modulus of rigidity. While the phenomenon of fatigue in the elasticity of stretching is much less marked, it gives us, nevertheless, a new fact concerning the properties of matter, and deserves careful attention.

The observations noted below were made in the spring of 1891 in the Physical Institute of the University of Strassburg. The object then immediately in view was to demonstrate the invalidity of Hooke's law, and to point out a method of determining the Young's modulus of an unstrained wire. Consequently the phenomena described below were unlooked for, and at that time received merely a passing glance. Then, on account of lack of time, no attempt was made to subject the phenomena to an

[1] Mathematical and Physical Papers, Vol. III., p. 26.

extended investigation; and since that time there have not been at the disposal of the writer place and apparatus suitable for experiments demanding such a high degree of accuracy.

The apparatus used was able to give exceptionally accurate results. Since all its details, together with the method of experiment, have been fully described in an earlier article,[1] extended description of the same is omitted here. Suffice it to say that the wires used were about 23 m. in length and 0.25 mm. in diameter; that the temperature was remarkably constant, and could be measured to $\frac{1}{80}°$; and that the constant use of two cathetometers enabled me to keep the true elastic lengthening separate from the elastic after-effect (*elastische Nachwirkung*) and all other disturbing influences.

A careful examination of my results showed that the elastic lengthenings in the first of any series of measurements were uniformly less than those caused by the same stretching weights later in the day, the temperature remaining either exactly or approximately the same. This is shown clearly in the table below, where the first column shows the weights added to the pan; the second, the lengthenings produced in an unfatigued steel wire, as given by the averages of three sets of observations; the third, the lengthenings produced in the fatigued wire, as given by the averages of six sets of observations: —

Kilograms.	Millimeters.	Millimeters.	Difference.
0.2	6.993	7.009	0.016
0.4	14.065	14.085	0.020
0.6	21.188	21.210	0.022
0.8	28.353	28.366	0.013
1.0	35.548	35.569	0.021
1.2	42.792	42.807	0.015

It cannot be said that these differences in the last column are due to a diminution of the Young's modulus on account of a rise in temperature; for, after each one of the three sets of measure-

[1] Wiedemann's Annalen, Vol. 44, No. 11, 1891, and Am. Jour. Sci., Vol. XLIII., January, 1892.

ments, the averages of which are given in the second column, I immediately took two other sets, and thus I get the six sets, the averages of which appear in the third column. In every such series of three sets of measurements, the temperature was entirely free from fluctuation. The rise during the first series was $\frac{1}{4}°$. It was the same in the second, and was $\frac{1}{20}°$ in the third; and in this third series the fatigue was as marked as in either of the others.

The probable error in the measurements was small. For example, when the weight added to the pan was 0.4 kg. the elastic lengthenings produced were as follows: —

	May 12.	May 13.	May 15.	Mean.
Unfatigued wire .	14.065	14.06	14.07	14.065
Fatigued wire .	14.08 14.08	14.08 14.09	14.085 14.095	14.085

The increase in the measurements, made May 15, is due chiefly to the fact that on that day the temperature of the tower in which the observations were made was nearly $\frac{1}{2}°$ higher than on the other two days.[1]

Brass wire gave phenomena exactly similar to those described above. In the following table are given the results of the first four sets of measurements, made April 25, between 8.58 and 10.57 A.M. The rise in temperature in this time was perfectly uniform, and amounted to $\frac{1}{4}°$; and, according to Dr. Miller, this would not lower the modulus by more than $\frac{1}{8000}$.

Kilograms.	Millimeters.	Millimeters.	Millimeters.	Millimeters.
0.2	7.10	.13	.12	.11
0.4	14.25	.265	.26	.28
0.6	21.47	.50	.50	.50
0.8	28.76	.775	.78	.77
1.0	36.11	.125	.14	.145
1.2	43.53	.57	.56	.55
1.4	51.05	.07	.065	.07
1.6	58.66	.71	.685	.71
1.8	66.33	.355	.335	.36

[1] According to the measurements of Dr. Andreas Miller, of Munich, an increase of $\frac{1}{2}°$ in temperature lowers the Young's modulus of steel by $\frac{1}{5000}$.

The lengthenings obtained in the first set are evidently less than the others. Whether the fatigue is progressive, that is, whether it is a function of the time the wire is kept in use, my measurements do not indicate.

A similar fatigue was distinctly noticed in a silver wire.

In copper wire the after-effect was troublesome, and obscured the phenomenon of fatigue, and indeed made it uncertain whether in this particular metal the phenomenon existed. I consider it probable, however, that it does exist, just as in the other metals mentioned.

In answer to the query, Why is the fatigue in the elasticity of stretching so comparatively slight, the following may be said: In Lord Kelvin's torsion experiments the elastic forces had been kept in almost constant action for several days. In my experiments, on the other hand, measurements were made at intervals of two minutes, and the stretching weight was allowed to remain on the pan only twelve or thirteen seconds, so that even when observations were being made the elastic forces were in action only 10 per cent of the time, and this was about 1 per cent of the entire day. Under these circumstances, it is hardly surprising that the fatigue is slight. It will be borne in mind that if I had kept the elastic forces continually in action, I should have had a superposition of after-effects, and this after-effect was the one thing which I was at that time particularly anxious to avoid.

Reference should here be made to an article by Dr. Miller,[1] entitled "Der primäre und sekundäre longitudinale Elastizitätsmodul und die thermische Konstante des Letzteren." Dr. Miller states that he found in his measurements that the lengthening of his wire was greater in his first measurement than in the succeeding ones, a result exactly the reverse of mine. Still he states that the Young's modulus of the wire in its original condition, or its primary modulus (E_p), as he terms it, is greater than its modulus after it has been subjected to repeated stresses, — its secondary modulus (E_s). This statement accords with my result, and is doubtless true, although his measurements give him no

[1] Aus den Abhandlungen der k. bayer. Akademie der Wiss., II., Cl. XV., Bd. III., Abth.

ground for making it. He correctly says that $E_p=\frac{\pi_1}{l_0}$ and $E_s=\frac{\pi_2}{\lambda}$, and then goes to work to prove that $E_p>E_s$. In his demonstration, however, he in one place makes the incorrect assumption that $E_s=\frac{\pi_1}{l_0+l'}$, where l is what he terms the *Nachwirkungsrückstand*, a lengthening due chiefly to the after-effect. So we would here remark, in the first place, that in computing the modulus, no one is justified in combining this l with the true elastic lengthening; and, in the second place, that if we assume $E_p=\frac{\pi_1}{l_0}$ and $E_s=\frac{\pi_1}{l_0+l'}$, we see at a glance that of necessity $E_p>E_s$ (l being in every case a positive quantity), and the author's laborious proof is superfluous.

The elastic lengthenings in my measurements are affected to an inappreciable extent by the after-effect, and not at all by the thermal effects within the wire, as has been demonstrated in my article, already cited.

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A STUDY OF THE POLARIZATION UPON A THIN METAL PARTITION IN A VOLTAMETER. II.

By John Daniel.

IN this paper two questions will be discussed: first, the passage of ions through a gold-leaf partition in a voltameter; second, the minimum current strength at which the ions are deposited visibly upon the partition for various electrolytes. This will be called the "critical current." This paper is a continuation of the work done last spring in Berlin in the quantitative measurement of the polarization upon metal partitions ranging in thickness from 0.0001 mm. to 0.02 mm. for various current strengths in a 30 per cent H_2SO_4 voltameter. In those experiments there was no development of gas nor polarization on a gold-leaf partition (0.0001 mm. thick) for the highest current used, which was four-tenths to five-tenths ampere.

The present apparatus consists essentially of a glass voltameter vessel with platinum electrodes separated by the metal partition under investigation, so that there is no path for the current except through this partition, an accurate instrument for measuring current, and a strong, steady battery. The voltameter consists of an outer glass jar 8 cm. high, 8 cm. wide, and 8 cm. long; and an inner glass jar 8 cm. high, 5 cm. wide, and 5 cm. long, placed inside the first jar. A platinum cathode, suspended by a platinum wire, is placed inside the inner jar; and a similar electrode serves as anode in the larger jar, though a copper anode was sometimes used when the electrolyte was $CuSO_4$. A hole 2 cm. in diameter was bored in one side of the smaller jar. Glass plates 4 cm. wide, 6 cm. long, and 1 mm. thick were bored with a hole 1.5 cm. in diameter. Powdered sealing-wax was placed around the edge of this hole and melted; the gold-leaf was secured to the melted

sealing-wax. This plate was then securely sealed over the hole of the inner jar, so that there was no possible leakage. The voltameter was then carefully filled, keeping the liquid inside and outside on the same level so as not to break the gold-leaf partition, and was then ready for use.

To measure the current, a Thomson composite electric balance and a Weston double-scale ammeter were used in series with the voltameter. The two instruments were found to agree so well that the Weston ammeter was used alone for most of this work, as it was much more convenient to read.

The battery consisted of 25 accumulators of 350 ampere-hours capacity, and an electromotive force of 50 volts. The current strength was adjusted to any desired value by resistance in series, which could be varied at pleasure between zero and 12,000 ohms. For sealing-wax, pure rosin and beeswax (without coloring-matter) was used, mixed in such proportion as to give a low melting-point. The gold-leaf used was bought in Nashville, and is known to the trade as "XX." It is about 0.0001 mm. thick. Careful selection was made of such parts of the gold-leaf as were found, upon holding up to the light, to be free from small holes. It has already been observed, in Part I. of this paper, that when $CuSO_4$ was used as the electrolyte, and the current was over 0.3 ampere, copper was deposited on the rim of the gold-leaf, which was necessarily larger than the hole in the glass plate. In this work it was found necessary to remove this gold quite close up to the edge of the hole by scraping it off, as was first tried, or by covering it carefully with sealing-wax, which was found both easier and better. This was accomplished by melting the sealing-wax over the gold with a hot brass hammer of peculiar shape made for the purpose. The wax could thus be made to flow quite close to the edge of the hole. This left only that part of the gold exposed which was immediately over the hole. To neglect this was in all cases to reduce the critical current, the deposit of the cathion appearing first on that part of the gold-leaf which was nearest the anode and farthest from the opening.

Passage of the Ions through the Gold-leaf Partition.

To test this, a 17 per cent $CuSO_4$ solution (*i.e.* 1 g. $CuSO_4$ to 5 c.c. H_2O) was used in the outside vessel (anode side of the voltameter), and H_2SO_4 (30 per cent solution, Sp. Gr. 1.23) in the inside vessel (cathode side of the voltameter). These solutions being separated by the gold-leaf partition, the appearance of the ions upon the electrodes and upon the partition, when the current was closed, was noted.

The first method of observation was to close the circuit upon the voltameter, read the current strength by the ammeter, and, at stated intervals, weigh the copper deposited on the cathode. This gave Table V., where it may be seen that the amount of copper deposited was very small at first, not more than 2 per cent or 3 per cent the first hour, but increased rapidly with the time. This did not settle the question as to whether the current caused the copper to pass through the gold-leaf partition or not.

The second method was to set up two exactly similar voltameters at the same time, close the circuit on one, leaving the other open, and, at stated intervals, to weigh the copper deposited on the cathode of the voltameter through which the current had passed, at the same time making a quantitative analysis of the solution on the cathode side of both the open and the closed voltameters. This was done by extracting 10 c.c. from each with a pipette, and depositing the copper electrolytically in two similar platinum crucibles, connected in series. Equilibrium was maintained in the voltameter by adding 10 c.c. of the 30 per cent H_2SO_4 solution to replace the 10 c.c. thus removed. Knowing the volume of solution in each voltameter, these analyses were sufficient to determine the total amount of copper that had passed through the partition during the same interval for each voltameter. Table VI. gives the results. Here it was observed that imperceptible differences in the specimens of gold-leaf were sufficient to cause enough difference in the diffusion to leave the question unsettled. The amount passing the partition of the open voltameter was as often *greater* than that of the closed voltameter as it was *less*. It was now evidently necessary to test one and the same gold-leaf

partition for diffusion, with circuit open and closed successively. This leads to the third method, the results of which are shown in Table VII.

It is thought that this method is entirely free from objection or serious error; it leads to the conclusion that the current does not sensibly affect the diffusion of $CuSO_4$ and H_2SO_4 through the gold-leaf partition. This method, in detail, was to set up two similar voltameters at the same time, leaving both open and mak-

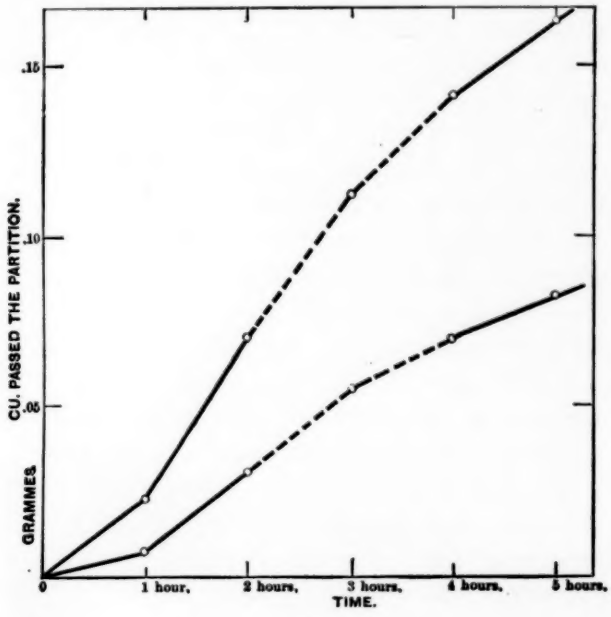

Fig. 5.

ing quantitative analyses at stated intervals; then, closing the current through both in series, to again make quantitative analyses of the solution on the cathode side, and to weigh the copper on the cathodes; finally, to break the circuit and again repeat the analyses. Curves plotted (see Fig. 5) from these results show *no break nor change of slope* for the intervals during which the current was passing. In the foregoing experiments there was no deposit of copper on the gold-leaf partition, provided the current was less than about 0.3 of an ampere. The time curves for the

two specimens of gold-leaf are not identical, showing a different rate of diffusion, but are quite similar in shape. Using a copper anode, there was no visible gas on the anode, but the anode was dissolved by the current in the usual manner. Using a platinum cathode and a platinum anode, there was, of course, an escape of gas from the anode proportional to the current strength. Gas always escaped from the platinum cathode, but proportionally less as the amount of copper deposited increased. In the case where the partition was removed and the solutions thoroughly mixed, there was no gas, but the full amount of copper deposited on the cathode. Thus it seems that the passage of the current in such a voltameter is not accomplished by the passage of the copper through the gold-leaf partition. Some of the $CuSO_4$ passes the partition by diffusion, and then does its proportional part of the conduction of the mixed solution of CuSO and H_2SO_4, which accounts for the increased per cent of copper deposited on the cathode, as the interval, from the time the voltameter was filled, increases.

In the time curves which may be constructed from Table VII. (see Fig. 5) it will be noticed that the amount of copper at the end of the first interval seems too small, causing a similar bend in both curves. This is doubtless due to the fact that the current was not continued long enough to deposit all the copper in the 10 c.c. sample. The amount of $CuSO_4$ present being very small, it would be deposited very slowly; and the circuit should have remained closed several hours instead of less than an hour, as was the case. A similar bend occurs in both curves, because the specimens were analyzed in series and the same defect applies to both alike. If the current was increased beyond about 0.3 ampere, with $CuSO_4$ and H_2SO_4 as the solutions, separated by a gold-leaf partition, there was a development of gas and copper on the partition. This brings us to the second problem, the determination of the *minimum current strength* at which the ions are deposited visibly on the gold-leaf partition for various electrolytes.

Critical Current Density for Gold-leaf.

This part of the investigation consists simply in closing the circuit on the voltameter with a high auxiliary resistance, slowly increasing the current by decreasing the resistance, and observing at what current strength bubbles of gas or deposit of metal *begin* to appear upon the partition. Table VIII. shows these values for various solutions used as electrolytes, with a gold-leaf partition. Table IX. gives the results in 30 per cent H_2SO_4 for aluminum-leaf, platinum-leaf, and gold-leaf, respectively. Table IX. shows the same for gold-leaf partition in H_2SO_4 of various concentrations from 100 per cent (?) to 0.5 per cent. Unfortunately the importance of the concentration of the solution in affecting the value of the "critical current" was not noted until after several of the solutions had been tested and thrown away without measuring their concentration or specific gravity, which therefore are not given for those cases. The specific gravity of those solutions for which it is given was determined by a very sensitive and accurate set of hydrometers reading directly to 0.001. Table IX. gives the results for 30 per cent H_2SO_4, with platinum partitions of various thicknesses; also of a "thick" platinum partition perforated with 1, 2, 3, and 4 holes successively, each 0.5 mm. in diameter. This is important in showing that the very small holes which may have existed in the carefully selected gold-leaves used as partitions, could not have had much, if even a sensible, influence on the value of the critical current. Table X. shows the interesting case of a 0.02 mm. paladium partition in 30 per cent H_2SO_4. The values given in the tables are in most cases the mean of several determinations which differed but slightly.

The importance of having the edges of the partition covered well up to the edge of the hole in the glass was emphasized in this work on account of the fact that the anode was usually placed on one side, and sometimes even at the back of the inner vessel, instead of in the front, in order not to obstruct a free inspection of the partition, and to avoid the disturbance of the gas escaping from the anode, as this was considerable with the strongest currents used; *i.e.* 10 to 12 amperes. Thus one edge of the

exposed partition being nearer to the anode than the opening, the deposit would always begin first on the nearest edge.

Inspection of the tables will show that in 30 per cent H_2SO_4 the gold-leaf partition will pass about ten amperes before gases are developed upon it. Platinum-leaf and aluminum-leaf showed

TABLE V.

A 17% SOLUTION OF $CuSO_4$ WITH COPPER ANODE SEPARATED BY A GOLD-LEAF PARTITION FROM A 30% SOLUTION OF H_2SO_4 WITH PLATINUM CATHODE.

Time in minutes.	Current in amperes.	Cu deposited on cathode in grams.		Ratio of Cu to current.		
30	0.175	0.0030		3%		
33	0.205	0.0765		58%		After standing from Saturday to Monday.
40	0.206	0.0920		57%		
40	0.127	0.0700		70%		
45	0.054	0.0380		80%		
		No. 1.	No. 2.	No. 1.	No. 2.	
30	0.132	0.0020	0.0766	2.6%	99.0%	Voltameters in series.[1]
30	0.147	0.0270	0.0850	31.0%	99.0%	After some hours.
52	0.152	0.0453	0.0950	47.6%	100.0%	
65	0.035	0.0022	0.0026	5.0%	5.7%	Fresh solutions.[2]

REMARKS. — [1] The partition in No. 2 broke before the current was closed. [2] Partition of No. 1, single gold-leaf; of No. 2, double.

sensibly the same "critical current," rather less for the aluminum, which was, however, thicker though not so free from holes.

A solution of CdI_2 showed regular deposit of both cadmium and iodine upon the partition for the weakest current to 0.001 ampere. This case was peculiar in having both ions *solids*, and indicated as a consequence that neither would pass through the gold-leaf partition.

Using 9 per cent $AgNO_3$ solution, Ag_2O_2 was deposited on the anode, but this came about by a secondary action at the anode; and though we have in this case also two solids deposited on the electrodes as the result of the electrolysis, one of them is not primarily a solid, and the critical current is of the order of magni-

TABLE VI.

TO TEST DIFFUSION OF 17% $CuSO_4$ AND 30% H_2SO_4 WITH CURRENT PASSING IN VOLTAMETER NO. 1, AND NO CURRENT PASSING IN NO. 2.

Time in minutes.	Current in amperes.		Cu on cathode.		Per cent of Cu to current.		Cu in 10 c.c. solution.		Total Cu passed the gold.	
	No. 1.	No. 2.	No. 1.	No. 2.	No. 1.	No. 2.	No. 1.	No. 2.	No. 1.	No. 2.
92	0.0505	0.0	0.0058	0.0	6.4	0.0	0.0020	0.0023	0.0308	0.0300
87	0.0530	0.0	0.0143	0.0	15.8	0.0	0.0040	0.0050	0.0643	0.0650
	Fresh solutions and single gold-leaf partitions.									
45	0.1500	0.0	0.0022	0.0	1.7	0.0	0.0043	0.0040	0.0550	0.0480
65	Broke.	0.0	—	0.0	—	0.0	—	0.0060	—	0.0760
18 hr.	—	0.0	—	0.0	—	0.0	—	0.0256	—	0.3172
	Fresh solutions and gold-leaf partitions.									
55	0.0770	0.0	0.0022	0.0	5.3	0.0	0.0014	0.0033	0.0190	0.0410
60	0.0770	0.0	0.0090	0.0	20.0	0.0	0.0035	0.0065	0.0540	0.0850
60	0.0790	0.0	0.0142	0.0	30.8	0.0	0.0038	0.0084	0.0797	0.1148

Began the above at 12 hr. 55 min. 30 sec.; closed at 3 hr. 30 min. 30 sec.

tude of that for other metallic salts. A similar case was found in that of $MnSO_4$ solution, where MnO_2 was deposited on the anode. A solution of lead acetate gave PbO_2 on the anode, but showed a very low "critical current."

An interesting fact is the dependence of the "critical current" upon the concentration of the electrolyte, as shown in Table IX., indicating that the "critical current" is proportional to the conductivity of the electrolyte. I have made some experiments to

determine whether variation of the temperature has the same effect upon the "critical current" as upon the conductivity, although the results are as yet hardly sufficient to justify a conclusion. I have been entirely unable anywhere to get a series of platinum partitions of thicknesses suitable to repeat the quantitative measurement of polarization which were made in Berlin for the gold plates. Table X. for 0.02 mm. palladium is interesting.

TABLE VII.

THIRD AND BEST METHOD.

Time in minutes.	Current in amperes.		Cu on cathode.		Per cent of Cu to current.		Cu in 10 c.c. solution.		Total Cu passed the gold.	
	No. 1.	No. 2.	No. 1.	No. 2.	No. 1.	No. 2.	No. 1.	No. 2.	No. 1.	No. 2.
Fresh solutions and gold-leaf partitions.										
60	0.0	0.0	0.0	0.0	0.0	0.0	0.0019	0.0006	0.0228	0.0075
60	0.0	0.0	0.0	0.0	0.0	0.0	0.0055	0.0024	0.0679	0.0306
Then the circuit was closed.										
45	0.10	0.10	0.0154	0.0060	35.0	13.7	0.0074	0.0036	0.1116	0.0540
45	0.10	0.10	0.0205	0.0100	47.0	22.8	0.0080	0.0038	0.1467	0.0701
Again, leaving the circuit open, the following was obtained: —										
60	0.0	0.0	0.0	0.0	0.0	0.0	0.0090	0.0048	0.1667	0.0864
Filled the voltameters at 12 hr.; last test for Cu at 5 hr.										
16 hr.	0.0	0.0	0.0	0.0	0.0	0.0	0.0310	0.0187	0.5387	0.3200

Either gold, platinum, or aluminum as thick as this behaves like a very thick plate, *i.e.* shows gas at once in amount corresponding to the current strength. When first set up, the 0.02 mm. palladium partition showed a "critical current" of 0.08 ampere. After considerable use, the "critical current" has gradually, though not slowly, increased to 0.3 in 30 per cent H_2SO_4. This seems to be quite distinct from the well-known property of

palladium of occluding gases, especially hydrogen, for this plate will pass 0.2 amperes or more for hours without showing gas, and develops gas at once for a little stronger current. The gas on the partition of palladium will cease to escape if the current be again reduced below the "critical" value. Currents of several amperes have several times been passed through this partition, developing profuse gas, and it was thought probable that the palladium was

TABLE VIII.

SHOWING MEAN MINIMUM CURRENT AT WHICH GASES DEVELOPED VISIBLY ON A GOLD-LEAF PARTITION, WITH PLATINUM ELECTRODES.

Electrolyte.	$CuSO_4$.	$AgNO_3$.[1]	CdI_2.	$ZnSO_4$.	$MnSO_4$.[2]	$FeSO_4$.
Sp. Gr.	17%	9%	5%	saturated		1.10
Current	0.17	0.40	0.0001 –	0.19	0.15	0.32
Current density . . .	0.10	0.23		0.11	0.08	0.18

Electrolyte.	$SnSO_4$.	$NiSO_4$.	$HgSO_4$.	NH_4NO_3.		
Sp. Gr.				1.05	1.10	1.135
Current	0.30	0.13	0.17	0.65	2.80	3.00
Current density . . .	0.17	0.07	0.10	0.37	1.60	1.75

Elec.	Benzoate of Na.	Sulphate of		Acetate of			
		Am. & Fe.	Am. & Ni.	Pb.[3]	Am.	Am.	Na.
Sp. Gr. . .				1.10	1.06	1.09	sat'd
Current . .	0.75	0.18	0.31	0.018	1.10	0.70	0.75
Cur. density .	0.43	0.10	0.18	0.01	0.63	0.40	0.43

REMARKS. — [1] AgO_2 formed on anode. [2] MnO_2 formed. [3] PbO_2 formed.

A 96% solution of acetic acid was found to have too high a resistance to give a sensible current with 50 volts, electromotive force. Benzoic acid dissolved in the same was also an insulator.

being dissolved by the 30 per cent H_2SO_4 under the action of the current. Accordingly a test of the solution was made for palladium by adding ammonia until alkaline, and then adding ammonium sulphide, which gave no indication of the presence of palladium.

It has been observed that platinum is perfectly constant, showing the same "critical current" as often as tested. Gold is oxidized the first time the "critical current" is attained, and its value decreases on repeating the test (with the same specimen).

Efforts were made to test the critical currents for four thicknesses of gold from 0.0004 mm. to 0.0018 mm., in ammonium nitrate, hoping that this would not oxidize the gold; but the partitions were oxidized by currents strong enough to develop gas.

TABLE IX.

THE CRITICAL CURRENT AS AFFECTED BY STRENGTH % OF H_2SO_4, AND NATURE OF PARTITION.

Strength.	100 (?)	55	43	30			22	15	4	0.5
Partition .	Au	Au	Au	Al	Au	Pt	Au	Au	Au	Au
Current .	0.55	4.5	8.0	8.0	10.0	10.0	8.0	6.0	1.8	0.23
Cur. dens.	0.31	2.57	4.57	4.57	5.71	5.71	4.57	3.43	1.03	0.13

30% H_2SO_4. CRITICAL CURRENT AFFECTED BY THICKNESS OF PLATINUM PARTITION IN MILLIMETERS AND BY PUNCTURES 0.5 mm. IN DIAMETER.

Thickners.	0.00333	0.0050	0.02				
Holes	—	—	0	1	2	3	4
Current	0.090	0.002	weakest	0.07	0.13	0.20	0.27
Current density .	—	—	—	0.04	0.07	0.115	0.154

TABLE X.

INCREASING CRITICAL CURRENT OF 0.02 mm. PALLADIUM IN 30% H_2SO_4.

Date.	January		February					
	31	31	4	8	17	17	18	18
Current	0.08	0.10	0.15	0.17	0.25	0.26	0.27	0.27
Current density	0.045	0.057	0.086	0.10	0.143	0.148	0.154	0.154

A new specimen of the same palladium is being tested, and is showing the same phenomenon of increase of the critical current. This last specimen was carefully selected, and has *no* holes. Its critical current at first was 0.016; after some use, it was 0.043 ampere.

I have begun to use a solution of sodium acetate, with the hope that it will not oxidize the gold. I can already say that it is at least very much less active than sulphuric acid. It also shows a

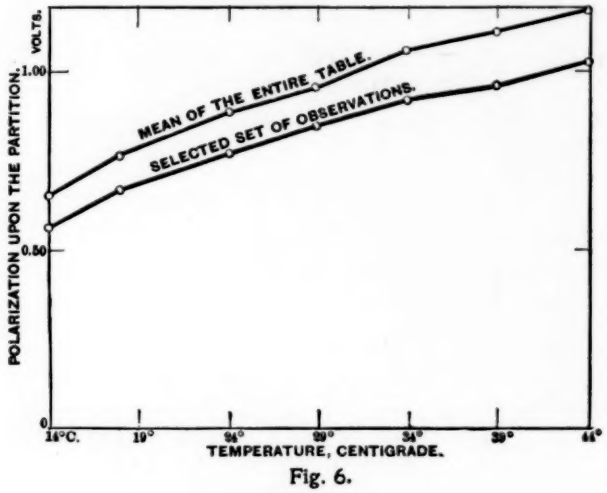

Fig. 6.

decided *temperature coefficient* (see Fig. 6) for the critical current. This part of the investigation will be resumed when opportunity offers.

VANDERBILT UNIVERSITY, NASHVILLE, TENNESSEE,
February 22, 1893.

NOTE. — More satisfactory specimens of palladium have not yet been obtained, so the matter is left for the present with the seemingly anomalous results, recorded above, as I find them in my note-book. As regards the temperature coefficient of the "critical current density," I have had Mr. R. W. Clawson, a student in the Laboratory, make some measurements, which are tabulated below. It may be well to state that the method of observation, that of

observing the first appearance of *visible* gas, is not very sensitive, because this development of gas on thin partitions is not quite sudden and sharply defined, but begins gradually, especially with very thin partitions. The probable error of a reading is, however, I think, not more than 5 per cent. Only "XX" gold-leaf has

TABLE XI.

(Double lines indicate a new partition.)

Sodium acetate; Sp. Gr. = 1.06. Gold-leaf partition.									
Temperature . . .	22	24	26	29	30	33	35	24	27
Current	.65	.70	.76	.80	.85	.92	.96	.53	.64
Temperature . . .	24	26	22	23	24	24	33	41	45
Current	.54	.60	.53	.57	.59	.77	1.18	1.33	1.48
Temperature . . .	15	17	19	20	23	26	30	33	37
Current	.30	.37	.45	.50	.65	.74	.83	.96	1.02
New solution sodium acetate; Sp. Gr. = 1.13.									
Temperature . . .	23	27	23	28	24	30	35	38	—
Current	.70	.80	.80	1.0	.95	1.05	1.20	1.29	—
Temperature . . .	14	19	24	29	34	39	44	—	—
Current	.57	.67	.78	.85	.93	.98	1.05	—	—
Temperature . . .	30	36	41	46	25	30	35	40	25
Current	.87	.91	.99	1.1	.71	.76	.86	.94	.60
Temperature . . .	30	35	40	45	25	30	35	40	45
Current	.65	.69	.74	.82	.72	.85	.92	.98	1.05
Temperature . . .	25	30	35	40	45	—	—	—	—
Current	.72	.85	.92	.98	1.05	—	—	—	—

been used in a solution of sodium acetate. The method was to set up the voltameter with gold-leaf partition, close the circuit, and run the current up until the "critical current density" was reached. Current and temperature were recorded, the current slightly reduced and allowed to pass till the temperature had

risen, say 5 degrees, when the current was again increased to the "critical" value for this new temperature. The limit was reached at about 45° C., when the sealing-wax softened. Since both "critical current density" and conductivity of the electrolyte increase with temperature, effort was made to learn whether both these quantities have the same temperature coefficient, by raising the current just to the "critical" value, and, without altering the outside resistance, to observe if the increasing current, due to increasing conductivity of the electrolyte, just sufficed to maintain an incipient escape of gas at the partition. In every case the gas soon ceased to escape on being left alone, after adjusting the current to a given temperature, requiring always some reduction of the outside resistance to regain the critical current for the higher temperature. A quantitative correction for the temperature coefficient of the outside metallic resistance in the circuit would probably account for the discrepancy.

The curve plotted from the mean of all the above results gives almost a straight line.

APRIL 15, 1893.

THE QUANTITATIVE DETERMINATION OF RADIANT HEAT BY THE METHOD OF ELECTRICAL COMPENSATION.[1]

By Knut Ångström.[2]

I.

THE older methods for the absolute determination of radiant heat, the methods of the pyrheliometer and actinometer, had two principal faults: they were neither accurate nor sensitive enough. They were, moreover, almost exclusively intended for the measurement of the radiation from the sun, and were not well adapted for the precise determinations frequently demanded at the present day in the physical laboratory. The great progress which has been made within the last fifteen years in the construction of instruments for relative measurement, such as the thermopile, bolometer, and the radiomicrometer, have been followed by very important investigations upon radiant energy. It is necessary to recall only the fundamental labors of Langley and the brilliant series of memoirs by Julius, Robert von Helmholtz, Rubens, Snow, and others. The investigation of radiant heat has taken an important place in the physical progress of the present day, and for that reason the need of a good method of quantitative measurement is strongly felt.

Such a method, by means of which relative measurements may be converted into quantitative ones by simultaneous observations of a given source of heat with two instruments, I described in 1885.[3] I have made use of the same continually since that time in my bolometric work, and can but emphasize its exactitude and reliability. In the comparative studies of different actinometric

[1] From the Transactions of the Royal Society of Sciences, Upsala, 1893.

[2] Translated for the Review by E. L. N.

[3] K. Ångström, Acta Reg. Soc. Ups., 1886. See also Bihang till K. Vet-Akad. Handlingar, 15, No. 10, 1889, and Wiedemann's Annalen, 39, p. 294, 1890.

methods carried on by Mr. Chwolson,[1] the advantages of this method have been clearly demonstrated. While, on the one hand, I regard, therefore, the problem of the quantitative measurement of radiation as solved, it must be said, on the other, that differential pyrheliometers leave something to be desired as regards simplicity and convenience of operation. In order to compare this, my older method, with the one to be described below, permit me to refer briefly to the former.

Two circular plates of massive copper serve as calorimeters; the blackened faces of the same are exposed alternately to radiation, and one observes the time at which the difference of temperature (θ) changes sign. This difference of temperature is determined by means of a thermo-element and a mirror galvanometer. If W is the water equivalent and A the absorbing surface of the calorimeter plates, then it is plain that the quantity of heat which is absorbed by a unit of surface is expressed by the equation

$$Q=\frac{2\theta W}{AT},$$

and that by this arrangement of observations the consideration of rate of cooling is eliminated.

II.

The principle of the new method is briefly as follows: Given two thin strips of metal, A and B, Fig. 1, which are as nearly as possible identical. The sides of these, which are exposed to the source of heat, are blackened, and the strips are arranged in such a way that it is possible to determine accu-

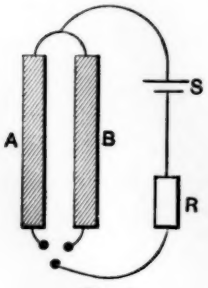

Fig. 1.

[1] O. Chwolson, Rep. für Meteorologie, 15, pp. 1–166; also 16, pp. 1–150. The former treatise contains a theoretical and critical discussion of actinometric procedure; the latter, a full account of my own method, including some attempts to devise a convenient apparatus, making use of the same, for meteorological purposes. Although both researches have as their chief aim the measurement of the radiation of the sun, they are of great interest from the point of view of general study of radiant energy. The success of the author's modifications of the method need not concern us here. Upon another occasion I hope to discuss both of these important papers in detail.

rately when they are of the same temperature. These strips are so placed that a current of any desired strength can be sent through them. If one of them, for example, A, is exposed to the source of heat, while B is protected by a screen, we may restore the balance of temperature which has been disturbed by the absorption of heat on the part of A, by sending a current of proper intensity through B. When the temperatures are the same, then the amounts of energy which A and B have received are equal to one another. Let l and b be the length and width of the strips, r the resistance of the same, and i the current. Then since the heat absorbed by A is the equivalent of that produced by the electric current in b, we may write

$$qlb = \frac{ri^2}{4\cdot 2}, \text{ or } q = \frac{ri^2}{4\cdot 2\, lb},$$

where q is the radiant energy received by a unit of surface. In order to counteract the inequality of the strips, they are interchangeable, B being illuminated and the current sent through A. In order to compare the two methods, the operations necessary in each to a complete measurement of radiation are tabulated below.

A. Constants to be determined once for all.

THE OLD METHOD. (*The Differential Pyrheliometer.*)	THE NEW METHOD. (*Method of Compensation.*)
1. Determination of the water equivalent of the calorimeter plates.	1. Determination of the electrical resistance of the strips.
2. Determination of the absorbing surface.	2. Determination of the absorbing surface.

B. Observations to be made during each experiment.

1. Determination of the time within which the temperature differences of the calorimeters change sign.	1. Adjustment of the two strips to equal temperature.
2. Calibration of the thermo-element in degrees.	2. Measurement of the current.

This comparison appears to me to be favorable to the new method. The measurement of resistance is more rapidly and conveniently carried out than the determination of the specific

heat of the plates of the calorimeters, and as regards the determinations which are to be made during each experiment, it is much easier to make the adjustment to equal temperatures than to carry out the tedious determination of the time. The last operation requires a more skilled observer. The determination of current strength is doubtless of about the same difficulty as the calibration of the thermo-element in degrees. Since both of these measurements are ultimately dependent upon the constant of the instrument for the measurement of current, it seems to me that here also the new method is to be preferred, because that instrument does not need to be of so high a degree of delicacy, and consequently its figure of merit is more easily determined.

III.

In the practical application of this principle of compensation, one may follow various methods. The equality of temperature may be determined in a variety of ways. If thermo-elements

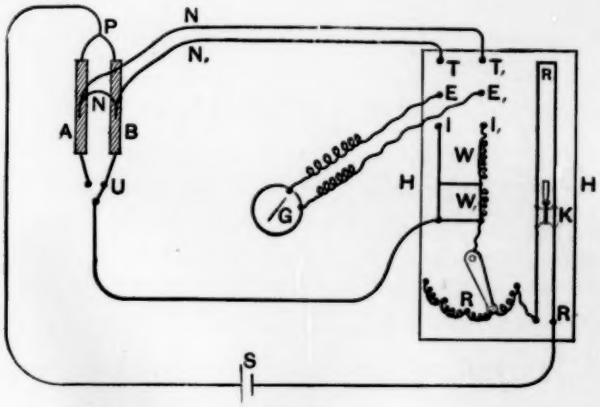

Fig. 2.

are used for this purpose, one needs a sensitive galvanoscope, and the measurement of the current can be made upon an instrument of ordinary delicacy. It is possible, however, to carry on the investigation without any difficulty, using only one galvanometer, a plan which I pursued in the case of the first apparatus which I constructed. Fig. 2 gives a diagram of the connections,

while Figs. 3, 4, 5, and 6 show more in detail the arrangement of the apparatus. The metallic strips, *A* and *B*, cut simultaneously from two thin sheets of platinum laid one upon another, are 0.154 cm. wide and 1.80 cm. in length. They are blackened in the usual way upon the side exposed to radiation, and are mounted

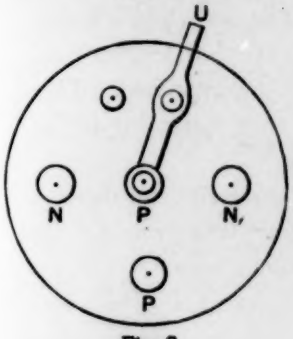

Fig. 3.

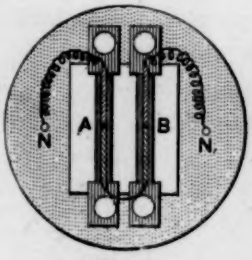

Fig. 4.

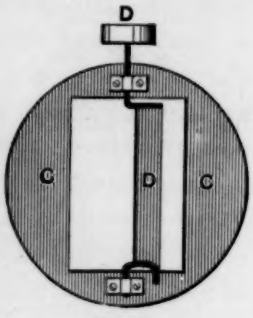

Fig. 5.

side by side in a frame of ebonite (Fig. 4). This frame is inserted in a tube, as is shown in Fig. 6. Upon the back of the strips are laid two exceedingly thin leaves of mica, upon which the very minute junctions of copper and German silver, NN_1, are inserted (see Fig. 4). To hold the mica and the thermo-junctions together, I used marine glue in as thin a layer as possible. Mounted upon

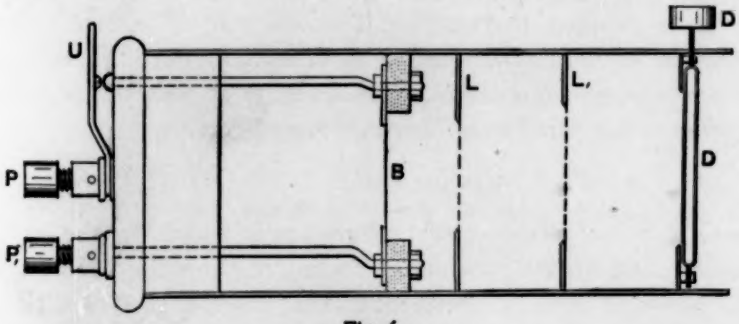

Fig. 6.

the block, Fig. 2, is a rheostat, a resistance WW_1, which serves as a shunt for the galvanometer *G*, and the commutator *TEI*. The rheostat has a sliding contact, *K*, by means of which the resistance can be varied at will. In this way the current from the battery, *S*, usually a Daniell's cell, is indirectly controlled.

One of the strips is subjected to radiation; the circuit is then closed through the other one; the thermo-element is brought into circuit with the galvanometer by means of the commutator; the sliding contact is adjusted until the galvanometer stands at zero; the commutator is then reversed, and the strength of the current used in heating the strip is determined. The switch, U (Fig. 3), is then reversed, the shutter is placed in front of the other strip, and the setting and current measurement are repeated. There are two other ways in which one can use this apparatus for the measurement of radiation, viz.:—

First Variation of the Method.—One of the strips, for example, A, is exposed to radiation, while the other is screened. One notes the deflection of the galvanometer, which becomes constant in about fifteen seconds. A is then also screened, and by means of the current is brought to the same temperature to which radiation had previously brought it. The strength of the current producing this rise of temperature is then measured. The advantage of this arrangement is that the same strip is warmed by means of radiation and then through the agency of the current, so that it is not necessary to be so painstaking as regards the identity of the two strips.

Second Variation of the Method.—One of the strips, for example, A, is exposed to radiation, the other, B, is screened; the deflection of the galvanometer is observed after the thermo-current has become constant. The quantity of heat which A has received is calculated from Newton's law of cooling,

$$Q = blq = k\theta,$$

where θ is the difference of temperature indicated by the galvanometer, and k is the constant of cooling of the strip. If we now send a current through A, without making any change of conditions, the difference of temperature will become greater still. This will be indicated by the resulting deflection θ_1. The strength of the current producing this additional heating effect may be measured in the manner already described. The amount of heat added to the strip is

$$Q_1 = blq + \frac{ri^2}{4 \cdot 19} = k\theta_1,$$

from which equations we find

$$q=\frac{i^2r\theta}{4\cdot 19(\theta_1-\theta)bl}.$$

When the experiment is performed in this way, it is not necessary to have an arrangement for the adjustment of the intensity of the current. The greatest advantage is, however, that the use of a screen may be avoided. This is perhaps the only method by means of which the screen, the use of which is always open to suspicion, can be eliminated.

IV.

As an example of the application of the new method, the following determinations of the radiation from an argand lamp may be of interest. In front of the lamp a double screen was set up so that the rays could reach the apparatus only through a hole 2.4 cm. in diameter. The distance between the strips and the middle of the flame was 60 cm. The other constants of the apparatus were as follows: Absorbing surface of each strip, .277 $\overline{\text{cm.}}^2$; resistance of A, 0.172 ohms; resistance of B, 0.176 ohms; reduction factor of the galvanometer (with shunt), 0.00122 amperes.

(*a*) *Experiment using the zero-method.*

Heating current of compensation:

(1) When A was subjected to radiation, 49.8 s.d.
(2) " B " " " " 49.7 "
Mean, 49.75 "

This reading corresponds to 0.0607 amperes, from which the radiation may be computed from the equation

$$q=\frac{\overline{0.0607}^2\times 0.174}{4.19\times 0.277},$$

$=0.000552$ gram-calories per second per $\overline{\text{cm.}}^2$

(*b*) *Experiment following the first modification.*

The strip A was subjected to radiation. The deflection of the galvanometer due to the thermo-current was 44.0 s.d. The strength of current necessary to bring about this rise of tempera-

ture was 49.5 s.d. = 0.0604 amperes, from which the following value of q was computed, viz.: —

$$q = 0.000541 \text{ gram-calories per second per } \overline{\text{cm.}}^2$$

(*c*) *Experiment following the second modification.*

The strip A was subjected to radiation. The galvanometer gave a deflection, due to the thermo-current, of 45.5 s.d. Upon closing the circuit of the heating current, the deflection rose to 91.5 s.d. The intensity of the current was 50 s.d. = 0.0610. The radiation was computed by means of the equation

$$q = \frac{\overline{0.0610}^2 \times 0.172 \times 45.5}{4.19 \times 0.277 \times 46.0},$$

from which we find $q = 0.000546$ gram-calories per second per $\overline{\text{cm.}}^2$

From what has been said it will be seen that the determination is an extraordinarily simple one. When the constants of the instrument are known and the figure of merit of the galvanometer, the whole measurement can be made with great exactness in a few minutes. If it is desired to take into consideration the change in resistance due to heating, it is only necessary, after an adjustment to zero, to open the circuit with the strip still exposed to radiation. From the deflection then produced, the temperature of the strip can be determined with sufficient exactitude. Thus far we have considered only the heat which is received by the instrument, without taking into account the absorbing power of the surface. In the method just described, as in the case of all other methods, it is necessary for exact measurements to determine the coefficient of absorption. No very good method for estimating this constant exists, but it is possible to obtain surfaces which possess the same absorbing power within the errors of observation; that is to say, within about 98 per cent. In most cases, therefore, the determination of this quantity may be neglected. In conclusion, I hope that the method outlined in this paper will be found to afford a means for the quantitative measurement of radiation, sufficiently simple, rapid, exact, and sensitive. In this brief note I have merely indicated the principle of the method; later, after a more thorough investigation, I purpose to give a fuller description of it.

MINOR CONTRIBUTIONS.

An Apparatus for the Generation of Oxygen and Hydrogen by Electrolysis.

By E. L. Nichols and G. S. Moler.

ALTHOUGH it is not possible to produce oxygen in large quantities by electrolytic action so cheaply as by other methods, there are certain conditions under which electrolysis is to be preferred. Whenever, for instance, it is desirable to have pure hydrogen gas instead of the illuminating gas which is ordinarily furnished, as well as oxygen, — that is to say, in our chemical and physical laboratories everywhere, where the gases are used not only in the oxyhydrogen blowpipe, but also for various analytical purposes, — the electric current will be employed.

Nearly all physical laboratories, at the present day, are provided with steam or water power and with shunt-wound dynamos, so that the production of oxygen and hydrogen by means of the current may be conveniently undertaken. The advantage of having a store of the pure gases at command is very great, not only for use with the lantern, but in a variety of other operations, for many of which the compressed gases furnished in cylinders do not suffice. For about seventeen years the Department of Physics in Cornell University has manufactured its own oxygen and hydrogen gas by electrolysis. It is the purpose in this paper to give a brief sketch of the various modifications which the methods there pursued have undergone, pointing out the technical difficulties which have been met and how they have been overcome, and then to describe, with sufficient detail to enable the present very satisfactory plan to be duplicated, the method at present in use.

Early Forms of Gas Generators. — In 1875 and 1876, Professor William A. Anthony constructed a series-wound Gramme dynamo for experimental purposes. This machine, which was probably the first of any considerable size made in this country, was exhibited at the Centennial Exhibition. In the following year experiments were made upon the production of gas for use with a magic lantern, in which experiments one of the present writers (Moler) took part.[1]

[1] For accounts of apparatus designed or in use elsewhere for the electrolysis of water on a large scale, see Giltay, Electrical Review (London), Vol. 18; Latchinoff, British

The first form of electrolytic apparatus consisted of twelve cells placed in series. The cells were of the following form: The lower part of each was of wood well filled with paint, forming an oblong box with a bar across the middle. The upper edge of the box and the cross-bar were grooved, and into these grooves were cemented two inverted boxes filled with sheet lead (Fig. 1). In the top of the boxes were the necessary holes and tubes for filling with acidulated water and for carrying off the gases. The twelve cells were set side by side in a trough of water, it having been found that the heat generated by the current would otherwise melt the cement.

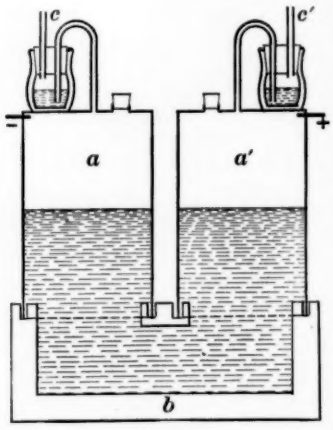

Fig. 1.

The twelve cells were connected in series in the circuit of the Gramme dynamo, and an automatic safety switch was placed in the circuit to prevent the counter-electromotive force of the cells from reversing the polarity of the dynamo when the latter was not in motion. This switch was so constructed as to shift the current to an outside wire of proper resistance whenever the current fell below a certain strength, and to shift it back again as soon as the electromotive force was great enough to overcome the electromotive force of the cells. Later on, the dynamo was separately excited, and this switch was dispensed with. Each leaden box was connected to an outside system of pipes, to which the gas was so conducted away through a separate wash-bottle. The bottles were necessary, in order that one might see that the cells, interiors of which were not visible, were working properly.

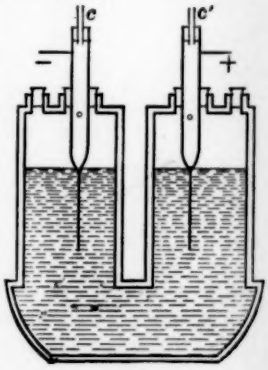

Fig. 2.

It was found to be impossible to keep the cells tight for any great length of time, and they were finally abandoned. Others, constructed of stone china, were tried with better results. The form of these china cells is shown in Fig. 2. They were shaped something like a U-tube with an enlarged base, being made in one piece. In the top of each branch of the cell were holes for corks, and through these the electrodes were inserted. These consisted of lead pipes, into the ends

Patents, Specifications of Inventions, Vol. 80, No. 15935 (1888); also, Fortschritte der Elektrotechnik, Vol. 4, p. 536; Renard, La Lumière Electrique, Vols. 39 and 40.

of which the platinum plates were fused. Two holes through the walls of the pipes allowed the gas to enter, and these pipes served as delivery tubes for the oxygen and hydrogen. They also conveyed the current to the platinum electrodes. The pressure within these cells was great enough to force the gas into gasholders, which were weighted down sufficiently to enable one to operate the jet of the lime light without further compression. This form of generator was in use for a considerable period, but was finally supplanted by a set of electrolytic cells made entirely of glass, the form of which is indicated in Fig. 3. The essential features of this, which is the type at present in use, are as follows: A tall cylindrical jar, six inches in diameter and thirty inches high, contains the acidulated water to be decomposed. Two long conical glass tubes, about one inch in diameter at the top and two and one-half inches at the base, are clamped side by side in a wooden holder, which rests upon the rim of the outer cylinder. These reach to within about two inches of the bottom of the latter. These tubes are fitted above with paraffined corks, through which lead pipes are inserted, care being taken to secure a thoroughly gas-tight joint. These pipes, as in the earlier forms, serve as conductors for the current and as delivery tubes for the generated gases. The electrodes are attached to the lower ends of these pipes.

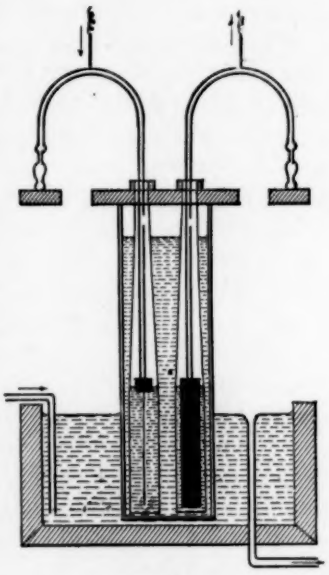

Fig. 3.

For a number of years platinum electrodes of the size already described were made use of, but it was found that upon the hydrogen side they suffered rapid disintegration, due to some local chemical action, the nature of which has not been investigated. On account of the expense and trouble of replacing these, experiments were made with the view of substituting other substances for platinum.

Following a suggestion by Renard, iron plates in an electrolyte consisting of a dilute solution of caustic soda were tried, but they were very short-lived. Carbon plates, such as are used in the cells of primary batteries, were also tested, the liquid being the usual acidulated water; but owing probably to the impure character of the carbon, these plates were found to disintegrate with great rapidity, especially while the generator was in operation, and they had to be abandoned. Lead plates were then tried, these

consisting of blocks of cast lead, into the end of which, while still molten, the extremity of the leaden delivery pipe had been inserted, forming a continuous piece. It was known that these would oxidize upon one terminal, forming a plate like that of the Planté battery, and that more or less of spongy lead would be produced at the other pole; but no further inconvenience than that arising from the very considerable counter-electromotive force was anticipated.

So much confidence was felt in the permanence and reliability of this form of electrode, that when a new generator was to be installed in the Chemical Laboratory of Cornell University, such leaden plates were introduced. The result proved to be disastrous. The new apparatus had been in use only a very short time when an explosion occurred of sufficient violence to destroy the entire generator. It was found upon investigation that the *oxygen* tank had been blown up, and that the mixing of gases which led to this explosion was due to an entirely unforeseen cause. When the current is passed through the leaden plates of such a generator, the electrode at the surface of which oxygen is set free is rapidly oxidized. This oxide is a good conductor of electricity. It is in great part adherent, but not entirely so; and in this case small portions became detached from the surface of the electrode and fell to the bottom of the cylindrical jar (Fig. 3). Thus in a short time a film was formed which covered the entire bottom of the cell. The conducting power of this layer was considerably greater than that of the electrolyte itself, and as a consequence double electrolysis took place. Hydrogen was generated at the hydrogen pole as before, and oxygen gas upon that portion of the film which lay immediately beneath the open mouth of the hydrogen-collecting tube. Hydrogen was generated again at that portion of the film which was situated under the mouth of the oxygen tube, and this mingled with the oxygen generated at the cathode of the apparatus. In this way mixed gases were delivered to both gasometers, and as soon as one of these reached the proper proportions for explosion and became ignited, the accident took place. Further study showed that this troublesome disintegration of the electrode was confined wholly to the oxygen pole, and that the spongy lead formed at the other terminal was completely adherent.

Now, as has been already stated, the difficulty with platinum electrodes lies with the plate at the surface of which hydrogen was generated, while the oxygen electrode remained bright and unimpaired. It became evident, then, that by using platinum plates as cathodes and lead plates as anodes, a generator could be constructed which would be free from the difficulties previously encountered with both of these metals; and when, in 1891, a new generator was to be installed in the Physical Laboratory, this plan was followed. The result was very satisfactory, the apparatus in question

having been in almost continuous operation for two years with entire freedom from the annoying, troublesome, and oftentimes dangerous experiences which had been met with in the attempt to use the earlier forms.

Test of the New Generator. — In June, 1893, a test of the new generator was made for the purpose of determining the efficiency of this method of producing oxygen and hydrogen gas. After the apparatus had been in continuous operation for a number of hours in order to bring it into its normal condition, measurements were begun.

At the beginning of the test the temperature of the cells and the density of the acidulated water which they contained were determined. The results are given in the following table: —

TABLE I.

Cell.	Temperature of electrolyte.	Density of electrolyte.	Cell.	Temperature of electrolyte.	Density of electrolyte.
1	30° C.	1.095	7	27° C.	1.085
2	31°	1.115	8	28°	1.080
3	28°	1.112	9	29°	1.095
4	32°	1.115	10	31°	1.130
5	34°	1.155	11	31°	1.090
6	30°	1.160	12	33°	1.149

According to this table, the average temperature of the electrolyte within the twelve cells was 30°.4 C., the coolest cell indicating 27°, and the hottest 34°. The average density of the electrolyte was 1.138, with a range in different cells from 1.080 to 1.160.

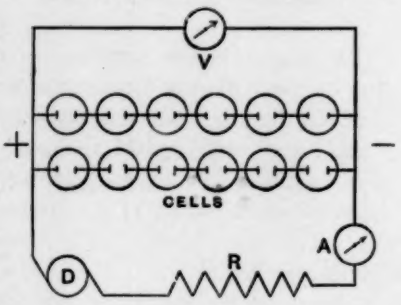

Fig. 4.

The temperature of the water entering the cooling trough surrounding the cells was found to be 21°.3; that of the water in the efflux tube was 23°. The rate at which water was supplied to the trough was determined by weighing that received during a given time (235 seconds). The amount was 60.34 kilograms, or 256.7 grams, per second.

The cells of the generator were connected in two groups of six, in parallel (see Fig. 4). They were in series with a rheostat (R); the whole

being in the circuit of a shunt-wound dynamo (D), giving 120 volts at the brushes. The rheostat was adjusted so as to secure a total current of about 49 amperes. A and V in the diagram are the ammeter and voltmeter.

The gases were collected over water in two large gasometers. These were partly filled previous to the beginning of the test, and the water through which the gases passed, as well as that within the generator cells, was thoroughly saturated. The amount of gas collected during the continuous run of 9000 seconds was computed from the vertical distance through which the tanks were lifted, their contents per centimeter of vertical height, the pressure, and the temperature. The following are the essential data: —

TABLE II.

Hydrogen tank; circumference (inside)	383.540 cm.
Hydrogen tank; area of cross-section	11706.000 sq. cm.
Hydrogen tank; total rise	28.880 cm.
Hydrogen tank; gas received in 9000 seconds	338.071 cu. cm.
Oxygen tank; circumference (inside)	384.050 cm.
Oxygen tank; area of cross-section	11733.000 sq. cm.
Oxygen tank; total rise	14.450 cm.
Oxygen tank; gas received in 9000 seconds	169.541 cu. cm.
Temperature of H gas, within the tank	24°.0 C.
Temperature of O gas, within the tank	24°.0 C.
Pressure of H gas, within the tank	1 atm. + 2.683 cm.
Pressure of O gas, within the tank	1 atm. + 2.683 cm.

The identity in the temperature of the two gases was doubtless the result of their passage through the water of the gasometers, which, standing within the two tanks, undisturbed for many months, in a room subjected to slight and very gradual changes of temperature, was naturally in the same condition in the two gasometers. The pressure was adjustable by means of water ballast in a cylindrical compartment at the top of each tank. The exact identity of the two readings for pressure is, of course, a mere coincidence.

Throughout the course of the test, frequent readings of voltage and total current were made, as follows: —

TABLE III.

Time.		Amperes.	Volts.	Time.		Amperes.	Volts.
1 h.	44 m.	48.8	115.0	3 h.	10 m.	49.1	114.0
1	54	49.3	116.0	3	18	49.0	114.0
2	09	49.0	115.0	3	24	49.2	114.5
2	15	48.7	114.5	3	38	49.4	114.5
2	25	49.0	115.0	3	47	48.6	113.5
2	35	50.0	117.0[1]	3	55	49.4	114.5
2	40	48.5	113.0[1]	4	05	49.5	115.0
2	46	48.8	114.0	4	15	49.0	114.5
3	00	50.0	116.0				

The average current was taken from these readings to be 49.2 amperes: the average voltage, 114.1 volts; and the total number of watts, 5614. The initial and final measurements of the positions of the gasometer cylinders were made at 1 h. 44 m. and 4 h. 14 m., respectively, which gave for the total length of run 2 h. 30 m. = 9000 s.

From the above data the capacity and efficiency of the generator are readily computed. Considering, for convenience, each pair of cells in the double series of six as a single cell, we have as the output per pair at 0° and 76 cm. (with correction for vapor tension): —

Liters of hydrogen in 9000 seconds 51.443
Liters of oxygen in 9000 seconds 25.721

Production in each of the twelve cells in 1 second: H, 2.8413 cu. cm.; O, 1.4290 cu. cm.

The ratio of these volumes is

$$\frac{51.443}{25.721} = 1.9940,$$

a value which is as near to the theoretical ratio as one could expect with any ordinary water voltameter.

The volumes of gas which should be produced by 49.2 amperes of current in 9000 seconds, assuming the electro-chemical equivalent of hydrogen to be 0.0000104 grams per second per ampere, are: —

Hydrogen: 52.110 liters in 9000 seconds;
Oxygen: 26.055 liters in 9000 seconds.

The agreement of the observed volumes with these values is within the errors of our knowledge of the pressure and temperature of the gases.

[1] A temporary value holding only for a few seconds.

The Efficiency of the Generator. — The efficiency of the generator may be readily computed from the mechanical equivalent of the heat of combination of the gases which are produced. Assuming the heat of combustion of hydrogen to be 34,200, we get as the output of the generator 48.74 kilogrammeters per second. The energy corresponding to 5614 watts is, however, 572.3 kilogrammeters per second, the ratio of which gives for the efficiency of the apparatus —

$$\frac{48.74}{572.3} = 0.0851.$$

This is a lower efficiency than that obtained by Professor Anthony with the porcelain cell generator in 1884.[1] He obtained, with an expenditure of 3104 watts, gas enough to account for 429 watts, which corresponds to an efficiency of 0.138. With a much smaller output the efficiency rose to 0.24. The test which has been described in this paper was purposely made with the apparatus at its maximum of production, which is the condition of its daily use.

No attempt was made to make determinations of the losses, but roughly speaking, they may be accounted for as follows: —

Estimating the amount of water delivered to the cooling trough and its rise of temperature, from data given in a previous paragraph, we find a large amount of energy taken away. During the test, also, the temperature of the electrolyte rose rapidly in spite of the cooling bath which surrounded the lower portions of the cells. Some time previous to the close of the experiments measurements showed the various cells to have risen to temperatures between 50° and 60°. The amount of water in the twelve cells was approximately 157 kilograms, and it is probable that the average rise in temperature was about 30°.

We may therefore estimate roughly the expenditures as follows: —

(1) Expended in heating the cooling bath	212	kgm. per second
(2) Expended in heating the electrolyte	222	kgm. per second
(3) Expended in generating gas	48.74	kgm. per second
Total	482.74	

This estimate leaves only about one-sixth of the total energy unaccounted for, an amount which would probably be found in the loss of heat by various parts of the apparatus to the surrounding air.

Under the conditions which these results indicate it is not strange that the manufacture of oxygen and hydrogen, which at one time appeared to be likely to take on commercial importance, has been superseded by other and more economical methods.

[1] Anthony, Proceedings of the American Association for the Advancement of Science, Vol. 33, p. 115 (1884).

June, 1893.

The Freezing-Points of Dilute Solutions. — A Correction.

By E. H. Loomis.

In the curves of results published on page 287 of the preceding number of the Physical Review in connection with the article on the "Freezing-Points of Dilute Solutions," the curve which gives the *theoretical* values for magnesium-sulphate was found to be incorrect after the proofs were returned to the publisher. It was thought better to indicate the error in a subsequent number of the Review than to change the curve itself.

The correct numerical data from which the curve was inaccurately constructed are as follows: —

m	$\frac{\Delta}{m}$
0.01	2.98
0.025	2.81
0.05	2.69
0.10	2.59
0.25	2.46

The Effect of Extreme Cold on Magnetism.

By Louis W. Austin.

Some years ago Professor Trowbridge[1] stated, in an article in the *American Journal of Science*, that steel bars magnetized at ordinary temperatures lose the greater part of their magnetism when subjected to the temperature of solid carbon dioxide and ether. This statement has remained, as far as I know, until now unchallenged. A recent experiment carried on in the Physical Laboratory of the University of Wisconsin would, however, seem to place it in doubt.

This experiment was as follows: A steel knitting-needle, 22 cm. long and 0.16 cm. in diameter, was magnetized to saturation, and, after being smartly tapped a few times to rid it of its temporary excess of magnetism, was placed in a wooden trough, being sprung into place in such a way as to be held fast by its ends about half-way between the bottom and the top. This was placed at right angles to the magnetic meridian in front of a magnetometer, whose deflections were read by the usual mirror and scale method. The trough was then filled with solid carbon dioxide, and ether was poured upon it.

Three series of observations were taken.

The first time that the freezing mixture was applied there seemed to be a slight decrease (about 1 per cent) in the strength of the magnet.

[1] J. Trowbridge, American Journal of Science, Vol. 21, p. 316 (1881).

The results in this first series were very uncertain, however, on account of the fatigue of the silk suspension of the magnetometer needle. This was, therefore, replaced in the other two series by a quartz fiber, which proved much more satisfactory in its action.

The results of the third series, which agreed almost perfectly with the second, are given in the following table : —

Time.	Deflections.	
3 o'clock, 12 min.	39.9 cm.	Magnet in place.
13	40.0	CO_2 applied.
15	40.1	Ether added.
17	40.4	
20	40.4	
25	40.5	
30	40.6	
35	40.7	
40	40.9	
4 o'clock, 00	41.3	Freezing mixture removed.
15	41.3	

At the end of these observations the zero of the magnetometer was found to have moved from 0 to 0.6 cm. This was probably due to the change in the magnetic condition of the building on account of the cooling of the iron steam pipes. The results would indicate that the magnetic moment of a permanent magnet, magnetized at ordinary temperatures, becomes slightly greater when the temperature of the magnet is reduced to that of solid carbon dioxide and ether.

These conclusions are in marked contrast to those of Professor Trowbridge, who found that a bar magnetized at 20° C. lost 66 per cent of its magnetic strength after being placed for forty-three minutes in a bath of the same freezing mixture as that used in my experiment.

PHYSICAL LABORATORY OF THE UNIVERSITY OF WISCONSIN,
January, 1894.

NOTE.

Heinrich Hertz. — On the first of January, 1894, died Heinrich Hertz, foremost of the younger physicists of Germany. Hertz was born in Hamburg in 1857. He received his early training in the engineering schools, but at the age of twenty-one he decided upon an academic career, and entered the University of Berlin as a pupil of von Helmholtz and Kirchhoff.

Of the many gifted students of physics who have come forth from the celebrated Berlin laboratory, there are no others who have become so famous as Heinrich Hertz became before the end of his too short life. His qualities as an investigator were speedily recognized by von Helmholtz, who urged him, while still a student, to undertake the solution of the prize problem proposed by the Berlin Academy of Sciences in 1879.

The subject presented by the Academy was *the experimental establishment of a relation between the electro-dynamic forces and the dielectric polarization of insulators.* The solution seemed at that time unattainable, and Hertz selected for his thesis, presented in the following year upon receiving the degree of doctor of philosophy, another subject, viz. *Induction in Rotating Spheres.* The fact that his attention was called to the Academy theme, although fruitless at the time, was to be of the greatest significance; for it appears that he carried the matter in his mind continually for seven years,[1] and ultimately built upon it, as it were, his crowning work.

From 1880 to 1883 Dr. Hertz was an assistant in von Helmholtz's laboratory; he then lectured for two years as *privat docent* at Kiel. From 1885 to 1889 he was professor of physics in the Polytechnicum of Karlsruhe. In the latter year Clausius died, and Hertz was selected as his successor in the University of Bonn, where he spent the few remaining years of his life.

Hertz's career as an investigator covers a period of scarcely more than ten years, during which time he published thirty-six papers. Of these, a series of thirteen, which appeared in *Wiedemann's Annalen*, were upon the subject with which his name will forever be connected, the laws of the propagation of electro-magnetic induction through space. Of this great work, which afforded a complete experimental verification of the Maxwellian theories concerning electro-magnetism and the relation of electricity to light, there is no need to speak. We have all witnessed the interest which it aroused, and have shared in the enthusiasm, unparalleled in the recent annals of physics.

[1] See the introduction to his work, Die Ausbreitung der Elektrischen Kraft, p. 1.

The importance of Hertz's contributions to this great subject received instant recognition. It would, indeed, be difficult to find any other instance in which researches bearing upon a most subtle and difficult question, and absolutely devoid of all elements of a utilitarian or even of a popular character, have secured to their author such sudden fame. In addition to the recognition of those who were able to appreciate his work, Hertz received the acclamations of the entire world of thought. Fortunately he possessed a nature of such complete simple-mindedness that his sudden rise into a position akin to notoriety had no effect upon him. The unassuming bearing which had always characterized him remained with him to the end.

In delightful harmony with the genuine and simple nature of the man were his surroundings in the quiet university town of Bonn. His laboratory was situated in the apartments formerly occupied as a dwelling by Clausius in a wing of the old palace. Since electricity has become utilitarian, we find it associated everywhere with moving machinery, and with the rush and bustle of modern industrial life, but in Hertz's laboratory there was nothing to suggest electro-technics. The place seemed to breathe that spirit of academic repose which to the inmates of most of the electrical laboratories of the present day must have seemed to have vanished altogether from the world. What might not such a man, in such an environment, have been able to mature and bring to fruition had he lived!

In 1892, at the request of the publishers of *Wiedemann's Annalen*, Hertz's researches upon electrical waves were gathered together in a volume, under the title "Untersuchungen ueber die Ausbreitung der Elektrischen Kraft." These papers, as they appeared, had been translated into English by De Tunzelmann. They were published in the *Electrician*.[1] Almost on the day of his death another excellent translation of Hertz's researches appeared. This volume[2] presents them in their final form, with the interesting introduction written by the author for the German version, a preface by the translator, Professor D. E. Jones, and a note by Lord Kelvin.

[1] The Electrician. London. Vols. 21 to 25.

[2] Electric Waves: being researches on the propagation of electric action with finite velocity through space. By Heinrich Hertz. (Authorized English translation by D. E. Jones, B.Sc.) Macmillan, 1893.

The following is a chronological list of the works of Heinrich Hertz:—

1880.

1. Ueber die Induction in rotirenden Kugeln. *Inaugural Dissertation*, Berlin, 93 pp.
2. Versuche zur Feststellung einer oberer Grenze für die Kinetische Energie der electrischen Strömung. *Wiedemann's Annalen*, 10, p. 414.

1881.

3. Ueber die Vertheilung der Electricität auf der Oberfläche bewegter Leiter. *Wiedemann's Annalen*, 13, p. 266.
4. Obere Grenze für die Kinetische Energie der bewegten Electricität. *Wiedemann's Annalen*, 14, p. 581.

1882.

5. Ein neues Hygrometer. *Physikal. Gesellschaft Berlin*, pp. 18–19 (1882).
6. Ueber die Berührung fester elastischer Körper. *Crelle's Journal*, 92, pp. 156–171.
7. Ueber die Härte der Körper. *Physikal. Gesellschaft Berlin* (1882).
8. Ueber die Spannung des gesättigten Quecksilber Dampfes. *Physikal. Gesellschaft Berlin* (1882); *Wiedemann's Annalen*, 17, p. 193.
9. Ueber die Berührung fester elastischen Körper. *Verein zur Beforderung des Gewerbfleisses*, November, 1882. [Contains a recapitulation of No. 5, and experiments in verification of the results therein obtained.]

1883.

10. Ueber die Vertheilung der Druckkräfte in einem elastischen Kreiscylinder. *Zeitschrift für Math. Physik.*, pp. 125–128 (1883).
11. Ueber die continuirlichen Ströme, welche die Flutherragende Wirkung der Gestirne im Meere veranlassen muss. *Physikal. Gesellschaft Berlin* (1883).
12. Dynamometrische Vorrichtung von geringem Widerstand und verschwindender Selbstinduction. *Zeitschrift für Instrumentenkunde*, 3, p. 17.
13. Eine Erscheinung beim Ueberschlagen des Funken eines Inductoriums oder einer Leydener Flasche durch mässig verdünnte trockene Luft. *Physikal. Gesellschaft Berlin* (1883); *Wiedemann's Annalen*, 19, p. 78.
14. Versuche über die Glimmentladung. *Wiedemann's Annalen*, 19, p. 782.
15. Ueber das Verhalten des Benzins als Isolator und als Rückstandsbildner. *Wiedemann's Annalen*, 20, p. 278.

1884.

16. Graphische Methode zur Bestimmung der adiabatischen zustandsänderungen feuchter Luft. *Meteorologische Zeitschrift* (1884), pp. 421–431.
17. Gleichgewicht schwimmender elastischen Platten. *Wiedemann's Annalen*, 22, p. 449.
18. Beziehungen zwischen den Maxwell'schen elektrodynamischen Grundgleichungen und den Grundgleichungen der gegnerischen Elektrodynamik. *Wiedemann's Annalen*, 23, p. 84.

1885.

19. Dimensionen des Magnetischen Pols in verschiedenen Maasssystemen. *Wiedemann's Annalen*, 24, p. 114.

1887.

20. Sehr schnelle electrische Swingungen. *Wiedemann's Annalen*, 31, pp. 421 and 543.
21. Ueber einen Einfluss des Ultravioletten Lichtes auf die Electrische Entladung. *Berliner Berichte* (1887), pp. 487–490; *Wiedemann's Annalen*, 31, p. 983.
22. Ueber Inductionserscheinungen hervorgerufen durch die elektrischen Vorgänge in Insolatoren. *Berliner Berichte* (1887), pp. 885–896.

1888.

23. Ueber die Ausbreitungsgeschwindigkeit der Elektrodynamischen Wirkungen. *Berliner Berichte* (1888), pp. 197–210; *Wiedemann's Annalen*, 34, p. 551.
24. Ueber Strahlen elektrischen Kraft. *Berliner Berichte* (1888), pp. 1297–1307.
25. The Forces of Electric Oscillations, treated according to Maxwell's Theory. *Nature*, Vol. 38, pp. 402–404, 450–452, and 547–548.
26. Einwirkung einer geradlinigen Schwingung auf eine benachbarte Strombahn. *Wiedemann's Annalen*, 34, p. 155.
27. Inductionserscheinungen durch die electrischen Vorgänge in Isolatoren. *Wiedemann's Annalen*, 34, p. 273.
28. Electrodynamische Wellen im Luftraume und deren Reflexion. *Wiedemann's Annalen*, 34, p. 609.

1889.

29. Ueber die Beziehung zwischen Licht und Electricität. 27 pp. Bonn, E. Strauss.
30. Electrische Schwingungen nach der Maxwell'schen Theorie. *Wiedemann's Annalen*, 36, p. 1.
31. Strahlen electrischer Kraft. *Wiedemann's Annalen*, 36, p. 769.
32. Fortleitung electrischer Wellen durch Drähte. *Wiedemann's Annalen*, 37, p. 395.

1890.

33. Ueber die Grundgleichungen der Electrodynamik für ruhende Körper. *Göttinger Nachrichten*, pp. 106–149; *Wiedemann's Annalen*, 40, p. 577.
34. Grundgleichungen der Electrodynamik für bewegte Körper. *Wiedemann's Annalen*, 41, p. 369.

1891.

35. Mechanische Wirkungen electrischer Drahtwellen. *Wiedemann's Annalen*, 42. p. 407.

1892.

36. Ueber den Durchgang der Kathodenstrahlen durch dünne Metalschichten. *Wiedemann's Annalen*, 45, p. 29.

E. L. N.

NEW BOOKS.

Utility of Quaternions in Physics. By A. McAulay, M.A., Lecturer in Mathematics and Physics in the University of Tasmania. pp. xiv, 107. London, Macmillan & Co.

The volume before us is an essay that was submitted in December, 1887, in competition for the Smith's Prizes at the University of Cambridge, under the title of "Quaternions as a Practical Instrument of Physical Research." An article bearing the original title of the essay appeared in the *Philosophical Magazine* for June, 1892, and another extract was printed in the *Proceedings of the Royal Society of Edinburgh*, 1890–91, p. 98, under the title of "Proposed Extension of the Powers of Quaternion Differentiation." The present volume contains the complete essay, with a short preface and some foot-notes in addition.

The essay opens as follows: "It is a curious phenomenon in the History of Mathematics that the greatest work of the greatest mathematician of the century which prides itself upon being the most enlightened the world has yet seen, has suffered the most chilling neglect." In further description of this phenomenon, it is stated that the work has been neglected alike by pure analysts and mathematical physicists with very few exceptions, the grand exception being Professor Tait; that it is not studied at the University of Cambridge except by a few, and only as a non-commutative algebra, not as a geometrical method; that there is a solid and well-nigh universal scepticism as to its utility in original physical investigations, and that the physicists who have studied it are satisfied with Maxwell's paradoxical position: "I am convinced that the introduction of the ideas, as distinguished from the operations and methods of quaternions, will be of great use to us in all parts of our subject." To complete the description of the phenomenon, I may add that a Scottish mathematician, on reading Hamilton's *Quaternions*, first formed the alternative conclusion that either he himself was a dull stupid or the book sheer nonsense, but on reading further was able to arrive at the more comforting alternative; that a German mathematician declared the method to be "an aberration of the human intellect"; and that a French mathematician gave the verdict, "Quaternions have no sense in them, and to try to find for them a geometrical interpretation is as if one were to turn out a well-rounded phrase, and were afterwards to bethink oneself about the meaning to be put into the words."

How does the author explain the curious phenomenon? As follows: "Workers naturally find themselves, while still inexperienced in the use of quaternions, incapable of clearly thinking through them and of making them do the work of Cartesian geometry, and they conclude that quaternions do not provide suitable treatment for what they have in hand. They then grow rather disgusted with these vexatious quaternions, and, consoling themselves with the reflection that Maxwell, before penning the above extract, had had more experience than themselves, decide that the subject only requires a superficial study to be rendered of as great utility as it is capable." But the author admits that there is a veritable stumbling-block in the way, and to remove it is the object of the essay. He says, p. 2: "The fact is that the subject requires a slight development in order readily to apply to the practical consideration of most physical subjects. The first steps of this, which consist chiefly in the invention of new symbols of operation and a slight examination of their chief properties, I have endeavored to give in the following pages." The author's development consists in an extension of quaternion differentiation, pp. 12–24. According to Hamilton and Tait, the symbol $\nabla = i\frac{d}{dx} + j\frac{d}{dy} + k\frac{d}{dz}$ is merely an operator, and therefore should be written immediately to the left of the operand; but, according to Mr. McAulay, it is a *symbolic vector*, and therefore is capable of any position, whether before or after the variable. He denotes the tie between the symbolic vector and the variable, not by juxtaposition to the left, but by a common suffix. In the third edition of his *Treatise*, Professor Tait allows separation, but he stickles for separation to the left only. A new symbolic vector Δ is introduced; it applies to all the variables in the term in which it appears. The symbol ∇ with a prefix, as $\sigma\Delta$, means $\frac{d}{du}i + \frac{d}{dv}j + \frac{d}{dw}k$, σ being $ui + vj + wk$. Similarly, ϕ being a linear vector function of any vector whose co-ordinates are $a_1b_1c_1$, $a_2b_2c_2$, $a_3b_3c_3$, $\phi\square$ is defined as a symbolic linear vector function, whose co-ordinates are $\frac{d}{da_1}\frac{d}{db_1}\frac{d}{dc_1}$, $\frac{d}{da_2}\frac{d}{db_2}\frac{d}{dc_2}$, $\frac{d}{da_3}\frac{d}{db_3}\frac{d}{dc_3}$. Finally, a symbolic vector ζ is introduced, which is such that $Q(\alpha, \beta)$ being linear in each of the vectors α, β,

$$Q(\zeta, \zeta) = Q(\nabla_1, \rho_1) = Q(i, i) + Q(j, j) + Q(k, k).$$

The remainder of the essay consists of an application of the quaternion analysis so developed to the theories of elastic solids, electricity and magnetism, and hydrodynamics. It is almost wholly a translation into quaternion notation of known results: the author, however, has endeavored to advance each of the theories chosen in at least one direction. The work

shown is designed to make good the following statements: *first*, that quaternions are in such a stage of development as already to justify the practically complete banishment of Cartesian geometry from physical questions of a general nature; and *second*, that quaternions will in physics produce many new results that cannot be produced by the rival and older theory. But in the preface, the author now states that he delayed publication until he could by a more striking example than any in the essay show the immense utility of quaternions; this he believes has been done by a paper published in the *Philosophical Transactions* for 1892. At the time of writing the essay he possessed little more than faith, and he felt that something more than faith was needed to convince scientists. In conclusion he exhorts mathematical physicists to study quaternions seriously, and he looks forward to the time when quaternions will appear in every physical text-book that assumes the knowledge of elementary plane trigonometry.

I agree with the author in his estimate of the value of Hamilton's quaternion researches: they constitute, in my opinion, the greatest mathematical work of the century. They contain what was long sought after — *a* veritable extension of algebra to space: I do not say *the*, for I believe that there is more than one. The Cartesian analysis is also an extension of algebra to space, but it is fragmentary and incomplete; whereas the quaternion analysis is the true spherical trigonometry in which the axis of an angle as well as its magnitude is considered.

But I cannot agree with the author in his explanation of the comparative neglect which the work has hitherto received. The notation has been a stumbling-block. Familiar functions, such as the cosine and sine, are replaced by new selective symbols S and V; Greek letters are used alike for axes, vectors, versors, and functions of these. As a consequence, the notation is contracted; to make it more expansive, Maxwell introduced German capitals for vectors, Mr. Heaviside used black letters instead, and in the work before us we have a rather incongruous mixture of Greek and black letters. The notation has divorced the method from the rest of analysis, so much so that Mr. McAulay believes that it is an independent plant which cannot be grafted on the old tree of analysis (*Philosophical Magazine*, Vol. 33, p. 479).

The identification of vectors and quadrantal versors has been a stumbling-block. Quaternion analysis is nothing but spherical trigonometry in which the axis of the angle is explicitly denoted; this will become clear to any one who studies the fundamental rules and the manner in which Hamilton arrived at them. It is the analysis of directed ratios. The identification mentioned assumes that it is also the analysis of lines, areas, volumes, and other physical products; it leads to the paradox that the square of a vector is essentially negative, and to a total disregard of the dimensions of physi-

cal quantities. Here no doubt we have the reason why Maxwell accepted the ideas but not the operations and methods. The analysis which the physicist mostly requires is not identical with, but complementary to, the analysis of ratios.

The largely *symbolic* character of the method has been a stumbling-block. Neither the definitions nor the rules have been placed on a clear and unambiguous basis. The fundamental rules are based, sometimes on the addition of quadrantal arcs, sometimes on the quadrantal rotation of an axis. The notation ∇ is presented as symbolic, so are the author's ∇, ζ, and $\square$; at most they are shorthand rather than systematic and logical notation. Need we wonder that competent mathematicians cannot think clearly through quaternions, for the original writers do not pretend to do so.

The rival or antagonistic attitude towards the Cartesian analysis has been a stumbling-block. Not only does the quaternion plant, according to Mr. McAulay, require independent sowing, but he would have us pull up the old Cartesian tree with its multitude of branches and far-spreading roots, in order to make room for the new plant. But when he comes to consider more specifically how much should be pulled up, he encounters a difficulty. Thus, p. 3, he says, "For particular problems, such as the torsion problem for a cylinder of a given shape, we require of course the various theories specially constructed for the solution of particular problems, such as Fourier's theories, complex variables, spherical harmonics, etc. It will thus be seen that I do not propose to banish these theories, but merely Cartesian geometry." If, then, the quaternion analysis fails, and the problem is turned over to the theory of complex variables (as at p. 49), it is important that these two branches of analysis should be logically harmonious and free from contradiction in matters of convention. If they are logically harmonious, it will be easy for a student or analyst to pass from the one to the other; but, as a matter of fact, the conventions are contradictory. Is not this the very meaning of the author's metaphor of the independent plant that cannot be grafted on the already flourishing tree? In several papers recently published I have aimed at showing how this logical harmony may be brought about, and one space-analysis be developed which shall embrace algebra, trigonometry, complex numbers, Cartesian analysis, Grassmann's method, and quaternions. Till this harmony is established the ideas and methods of Hamilton will not bring forth the great results which exist in them potentially.

ALEXANDER MACFARLANE.

Notes on Recent Researches in Electricity and Magnetism: intended as a sequel to Professor Clerk Maxwell's Treatise on Electricity and Magnetism. By J. J. THOMSON, M.A., F.R.S. Oxford, Clarendon Press, 1893.

The object of this volume is to give an account of the advances made in electricity and magnetism since the appearance of Maxwell's *Treatise;* thus the student may retain that treatise as the source from which to obtain the fundamental principles of the science and yet have a treatment which is thoroughly up to date. There is much in the book that is original; it opens with an entirely new view of the electromagnetic field, a physical theory which satisfies Maxwell's equations, and yet differs from the displacement theory. The importance and interest of the subject justify a brief sketch of it.

It is now accepted that if an electrified body moves through a dielectric, it produces the same external effects as an electric current. Of course tubes of electrostatic force — or, as our author calls them, Faraday tubes — terminate on this electrified body, and move with it. We have then a magnetic field accompanied, and apparently caused, by the motion of electrostatic tubes of force. May it not be that a magnetic field is nothing more nor less than the *motion of electrostatic tubes of force?* This is the essence of the view advanced by Professor Thomson.

The ether is supposed to be filled with Faraday tubes, which are regarded as incapable of creation or destruction. Some of these tubes form closed curves; others extend from an atom having a small positive charge to one having an equal negative charge. The existence of the closed tubes is necessary to account for the presence of a magnetic field in the absence of electrostatic charges, and moreover it is impossible to explain Rowland's celebrated experiment on convection currents without some such aid.

In speaking of the termination of a Faraday tube the word *atom* was used advisedly. All tubes have the same strength; the charge at each end is that which we associate with a univalent element in electrolysis. We certainly meet with no fractional part of this unit in electrolysis, and it is assumed we do not elsewhere. When the length of a Faraday tube joining two atoms is comparable with the distance between the atoms of a molecule, these atoms are said to be chemically united. The seat of an electric charge is always a dissociated atom, never a molecule. All the Faraday tubes the atoms of a molecule can accommodate are needed to hold them together in the molecule. The physical phenomenon of electrification differs from chemical combination only in the length of the Faraday tubes. In the case of liquids and gases there is good experimental evidence that the molecule cannot be charged, that the seat of a charge of electricity

always a dissociated atom. A digest of this evidence is given in Chapter II.

It is supposed that there is a motion of rotation inside and around the Faraday tubes, even when they themselves are at rest. The energy of this motion constitutes the potential energy of the electrostatic field. When the tubes are in motion, we have another form of energy, that of the magnetic field. Though these tubes are nowhere so called, one inevitably thinks of them as vortices. The interpretation of Maxwell's equations on this theory shows that a straight Faraday tube, moving at right angles to itself, generates a magnetic force at right angles both to itself and to its direction of motion.

Enough has been said to give a general idea of the theory. The reader must be referred to the book itself to learn how the inertia of the Faraday tubes causes an induced current, how, in case of two parallel currents, each shields the other from the bombardment of the tubes so that attraction results, the tubes playing a part similar to that of Le Sage's ultra-mundane particles. The action of a galvanic cell in producing a current is described, as well as the process by which light is transmitted. This resembles Newton's Emission theory, his symmetrical particles being replaced by Faraday tubes, thus making possible the explanation of polarization phenomena. This theory is of great value, not only because it gives a clear picture of what may be taking place in electromagnetic phenomena, but also because it sounds a loud note of warning against accepting any such view as an accurate representation of what actually does take place, as a photograph, so to speak, of the mechanism involved. In his *Modern Views of Electricity*, Oliver Lodge is guarded in his statement, speaking of electricity as "a form or rather a mode of manifestation of the ether." Yet doubtless more than ninety-nine out of every hundred of his readers have regarded the identity of electricity and the ether as practically proved. According to the view of Professor Thomson, electricity is the end of a tube of ether moving in a peculiar way; and to-morrow may bring forth another view which will fit the phenomena just as well. The theory advanced by our author is rich in promise, especially because it gives us a mental picture of chemical affinity.

Let us glance hastily at the rest of the book. In Chapter II. is an excellent summary of experimental work on the electrical behavior of gases; a field which is full of promise and has received altogether too little attention. Then comes a discussion of conjugate functions and of electrical waves and oscillations. Naturally following this is a description of the experiments of Hertz, and of the work which they have inspired. The literature of this subject is already bulky: hardly a number of Wiedemann's *Annalen* comes out without at least one paper which depends on the work of Hertz.

Such a summary as we have here, which gives the salient points of these researches in logical sequence, is of great value. Chapter VI. contains Lord Rayleigh's treatment of the distribution of rapidly alternating currents, and the last chapter treats of the electromotive intensity in moving bodies.

When possible, it is wise for the student to depend on libraries for many of his books; this volume does not belong to that class. In order of choice among electrical works it should come next to Maxwell's *Treatise*.

NORTHWESTERN UNIVERSITY. HIRAM B. LOOMIS.

Physikalisch-chemische Tabellen. Edited by HANS LANDOLT and RICHARD BÖRNSTEIN. Second Edition. Large 8vo, pp. xi + 563. Berlin, Julius Springer, 1894.

In spite of the great advances which have been made in all branches of physical and chemical science during the ten years which have passed since the first appearance of these well-known tables, this book has continued to maintain its position as the most useful and comprehensive of its class. Frequent revision in the case of a book of this kind is, however, a matter of absolute necessity. New and more reliable determinations of important constants are continually being made, and no book of tables which does not contain the results of these determinations can be in the highest sense useful for reference. It is not surprising, therefore, that the need of revision in the case of Landolt and Börnstein's Tables has for some time past been strongly felt. The great labor involved in the preparation and revision of a work of this kind is, however, a sufficient explanation of the delay in the appearance of the second edition. Indeed, were it not for the assistance of the seventeen collaborators whose names appear on the title-page the appearance of the book in its present form would doubtless have been altogether impossible.

In comparing the present edition with that of 1883, the reader will first be struck with the great increase in the size of the book. The number of pages has in fact been more than doubled. This increase has been brought about in three ways; viz. (1) the tables which appeared in the original edition have been enlarged by the addition of new data; (2) a number of new tables, dealing with subjects not treated in the first edition, have been added; (3) the number of references to the literature of each subject considered has been very largely increased. As stated in the preface, the book now contains not only a reference to the authority for each number given in the tables, but also references to numerous other determinations, whose results have for various reasons not been quoted. These references are given at the close of each table, or group of tables, and constitute by no means the least valuable part of the book.

To mention in detail the changes which have been made in the tables of the first edition would be a tedious and unnecessary task. Suffice it to say that the book has been thoroughly modernized by the insertion of the results of the most recent and reliable determinations. The following examples will serve to indicate how complete this revision is, and how greatly the tables have been enlarged.

In Table 1 (atomic weights) the results collected by Meyer and Seubert, by Ostwald, and by F. W. Clarke, are given in the tables proper, while the results of numerous determinations made in 1892–93 are given in the foot-notes. The atomic weights are referred both to $H = 1$ and to $O = 1$, thus enabling the reader to avoid the annoying uncertainty as to the atomic weight of oxygen. In the list of references following this table are collected the results of some twenty determinations of the atomic weight of oxygen, with a statement in each case of the method employed.

Tables 38–42, dealing with condensed gases, have been greatly enlarged. The latter table now contains not only the values of the critical temperature and pressure, but also the corresponding values of the volume and density. Data are given in this table for 150 substances. The following tables may also be mentioned as showing special enlargement: 74–84, on the Specific Gravity of Solutions and Mixtures; 118–121, on the Viscosity of Liquids and Gases; 142, 143, on Heats of Combustion; and 165, on Indices of Refraction for Organic Compounds. In the last two cases the increase is from two to fifteen pages, and from three to fourteen pages, respectively.

In Table 133, on the Specific Heat of Water, the recent determinations of Bartoli and Stracciati (1891) are added. The reader will be somewhat surprised, however, to observe that Rowland's results have been omitted.

The table which shows the most complete revision is perhaps No. 152, which deals with the Wave Lengths of the Spectral Lines. This table has been prepared by Professor Kayser, and is so thoroughly modernized that it might almost be considered new. For some ninety lines of the visible and ultra-violet spectrum, the results of Rowland's most recent determinations (1893) are given. A smaller table contains a comparison of the results of Fraunhofer, Ångström, Cornu, and Rowland. The value of this table naturally lies chiefly in its historical interest. Following these are tables containing the ultra-violet lines of iron (Kayser and Runge, 1890) and the more important lines of hydrogen (Ames, 1890). Next come lists of the principal lines in the spectra of the alkalies, and similar lists for thirteen other metals. These tables, which are highly satisfactory as far as they go, might well have been considerably enlarged. For example, the infra-red lines of the alkalies could to advantage have been added, while a table dealing with the absorption spectra of the rare earths, and in fact with absorption spectra in general, would have been in the highest degree useful.

Table 153, which has been prepared by Dr. Traube of Berlin and deals with the indices of refraction of isotropic substances, contains many additions which will be hailed with satisfaction. This table now contains, for example, the results of Kundt, Drude, and others for the indices of refraction of the metals. The results of Rubens and Snow for the dispersion of fluor-spar in the infra-red are given in detail, and on account of the many advantages which this substance has been shown to possess for work in the invisible spectrum, will doubtless prove valuable for reference. Determinations by Langley, Rubens, and Snow, and others of the dispersion of rock salt, have also been added.

In the tables dealing with electrical data the improvement due to revision is not so marked as in other parts of the book. The tables of electrical conductivity, although comprehensive, will be found inconvenient. The tables, with one exception, refer to conductivity, and not to resistance. Yet in nine cases out of ten it is the resistance which is required in computation. Again, all results are expressed in terms of the conductivity of mercury. A troublesome computation is therefore necessary in order to reduce to absolute or practical units, — a computation which is rendered more annoying by the fact that the absolute conductivity of mercury is nowhere conspicuously stated. The reader must either choose a value from those incidentally mentioned in the foot-notes, or be content with value of the absolute *resistance*, which is given to three places in Table 194.

An idea of the character of the additions that have been made in the present edition may be obtained from the following greatly abbreviated list of new tables: 12, Influence of g on Barometer Readings; 20–24, Constants of Capillarity for Various Liquids; 39–41, Melting and Boiling Points of Condensed Gases; 54, Temperature of Maximum Density for Various Salt Solutions; 87, Boiling Points of Solutions; 100, Various Constants connected with Electricity (*e.g.* Modulus and Coefficient of Elasticity, Modulus of Torsion, Elastic Limits, etc.); 126, Velocity, Free Path, etc., of the Molecules of Various Gases; 176, Constants of Electromagnetic Rotation; 188, 189, Molecular Conductivity of Solutions and Mixtures; 196, Specific Inductive Capacity. The last table has the disadvantage that the reader is left in doubt as to whether the results refer to rapid or to slow changes of charge. The table would be improved by a brief statement, after each result, of the method employed in obtaining it.

In considering the book as a whole, I think that most readers will agree that its chief fault — in fact, almost its only fault — lies in the arbitrary character of the units employed. In the preparation of the second edition the editors have had an excellent opportunity to adopt a uniform and logical system of units, as has so admirably been done in Everett's little book on "the C.G.S. System." This opportunity has, however, been neglected,

and units of all sorts and kinds appear in a most arbitrary way. The use of the C.G.S. system is, in fact, the exception rather than the rule. That in some cases the choice of units other than those of the absolute system is of advantage, cannot be denied. Yet when it is remembered that the book before us is intended for the laboratory rather than for the engineer, the number of such cases becomes very small. Attention has already been called to the annoyance that will be caused by the choice of units in the case of the tables dealing with electrical conductivity. Similar remarks may properly be made with regard to the tables on thermal conductivity, on elasticity, and in fact in connection with a large number of tables throughout the book. What possible advantage, for example, can be gained by expressing thermal conductivity in terms of millimeters and milligrams? In Table 100 we find the kilogram used as the unit of force, and results are in many cases carried to five places. Yet the value of g is not given. Is not this in the highest degree misleading and unscientific?

The results of this lack of uniformity are fortunately in large part counterbalanced by the clearness with which the units employed are defined. No uncertainty can exist as to the exact meaning of each number given in the tables. The importance of this statement can scarcely be overestimated, and that with many books it cannot truthfully be made is unfortunately too well known. Almost every one who has had to do with the properties of solutions has had occasion to puzzle over the expression "a 10 per cent solution." Does this mean 1 part salt to 9 parts water, or 1 part salt to 10 parts water? Can it be that the writer intended to say that the solution was one-tenth saturated? or is a 10 per cent normal solution referred to? The only way out of the difficulty is often either a happy guess, or an annoying search for the original authority. No difficulty of this kind will, however, be met with in Landolt and Börnstein's *Tables*. The arbitrary choice of units may often cause annoyance on account of the necessity of reducing to the absolute system, but no uncertainty can arise as to the meaning of the data given. A statement, in each case, of the factor by which the numbers given must be multiplied, in order to reduce to absolute units, would, however, be a great convenience.

In conclusion it is perhaps unnecessary to say that the book will be found useful to a degree which can hardly be estimated, and that to the editors, as well as to the numerous scientists who have assisted them in their arduous task, are due the thanks alike of physicists and chemists.

ERNEST MERRITT.

An Elementary Treatise on Fourier's Series, and Spherical, Cylindrical, and Ellipsoidal Harmonics, with Applications to Problems in Mathematical Physics. WILLIAM ELWOOD BYERLY, Ph.D., Professor of Mathematics in Harvard University. 8vo, pp. 287. Boston, Ginn & Co., 1893.

The following is a brief outline of the contents: Chap. I., pp. 1–29, Introduction; Chap. II., pp. 30–54, Development in Trigonometric Series; Chap. III., pp. 55–68, Convergence of Fourier's Series; Chap. IV., pp. 69–143, Solutions of Problems in Physics by the Aid of Fourier's Integrals and Fourier's Series, and Miscellaneous Problems; Chap. V., pp. 144–194, Zonal Harmonics; Chap. VI., pp. 195–218, Spherical Harmonics; Chap. VII., pp. 219–237, Cylindrical Harmonics; Chap. VIII., pp. 238–266, Laplace's Equation in Curvilinear Co-ordinates, Ellipsoidal Harmonics; Chap. IX., pp. 267–275, Historical Summary; Appendix, pp. 277–287, Tables. Chapter I. is rather uncommon in its style as an introduction to a mathematical treatise. It is, however, an exact reproduction[1] of the lectures given by Professor Byerly, as introductory to the course. It gives a list of the differential equations whose solutions are afterwards to be discussed, and brief statements of the general properties of homogeneous, linear, partial differential equations — particularly of the principle of superposition. By means of a few simple typical problems, which are discussed at some length, the student is fairly insinuated into the spirit of the methods for finding particular solutions, and for combining them by superposition so as to satisfy given conditions. It is a very unpretentious and exceedingly effective introduction.

Tentative methods only are given for finding particular solutions. This might be slightly improved by the statement, of which the proof is simple, that any homogeneous, linear, partial differential equation with constant coefficients, and which is free from terms of the form $\frac{d^n \cdot d^m}{dx^n \cdot dy^m}$, has particular solutions which are products of functions of the separate independent variables, and that such partial differential equations can always be degraded, for such solutions, to a number of ordinary differential equations from which the respective factors may be determined. It would be easy to show whether or not such method for finding particular solutions is generally applicable to this class of partial differential equations when transformed to orthogonal curvilinear co-ordinates. It seems that the method would be found general; and if so, we would thus arrive at the most general possible conception of the genesis of harmonic functions.

[1] The writer is so fortunate as to have heard part of the course in 1891–92, at which time Professor Byerly's notes were mimeographed for the use of the class.

The work as a whole is simple and straightforward in style, and easily within the reach of any student who has a moderate knowledge of differential and integral calculus. The simplicity of treatment is not of the degrading kind, however; and those students who would be likely under any circumstances to pursue a subject further than is possible in an elementary course will no doubt be left fresh and clear after a careful study of Professor Byerly's work.

Dr. Maxime Bôcher in Chapter IX. (historical outline), has given a set of choice references. Any references naturally lead the student into that branch of the subject which is of greatest mathematical interest; namely, into the formal study of the various harmonic functions. The conception of these various functions as actual distributions of potential, due to certain aggregations of positive and negative attracting points and lines, is almost as great a help to the physicist as is the conception of the various terms of Fourier's series considered as the different simple modes of vibration of a stretched string. For this reason, Maxwell's chapter on Spherical Harmonics is of particular interest to students of physics.

Professor Byerly's course is, on the whole, admirable, if we may by thus ignoring the title (treatise) emphasize the true nature of the work. We disagree with the author, however, in considering the work as introductory to mathematical physics. An introduction to that subject, indeed the subject itself, must eventually be, in essence at least, a discussion more or less philosophical of the origin and significance of physical quantity, of physical measurements, of the general theory of numerical specification, and of the transformation of physical definitions into differential equations and of physical laws into functional forms, leading to more or less complete methods for obtaining numerical data and for expressing mathematically the physical conditions they imply. Physical quantity, indeed, takes its origin from the application of differential equations and functional form, so that the above outline would seem to be not strictly systematic: but the order is chosen for the sake of emphasis. We cannot refrain from urging that mathematical physics, properly so called, is the only (formal) physics; that all problem work is essentially extraneous to it, and tolerated only because we know so imperfectly how to teach, and because students know so imperfectly how to understand. Mathematical physics, often so called, would be better characterized as physical mathematics.

W. S. FRANKLIN.

An Elementary Treatise on Theoretical Mechanics. By A. Ziwet. Part I., Kinematics. 8vo., pp. 8 + 181. Part II. Introduction to Dynamics: Statics. 8vo., pp. 8 + 183. New York, Macmillan & Co., 1893.

A text-book of elementary mechanics in three volumes octavo is something of a novelty. The custom of publishing books in parts is more common in Germany than in this country, and has some advantages.

The first two parts of the book have now appeared; the third part, devoted to kinetics, is promised for the fall of 1894.

The book is intended as an introduction to the Science of Theoretical Mechanics as such, but the author has constantly kept in mind the wants of engineering students more particularly. It assumes, on the part of the student, a good working knowledge of elementary mathematics, and of analytical geometry and the differential and integral calculus. An acquaintance with determinants, elliptic integrals, and the elements of quaternions is also required.

The arrangement of the matter is severely logical, all kinematical questions being disposed of before dynamics is taken up. The whole of Part I. is devoted to kinematics.

Part II. begins with a short introduction to dynamics, but the bulk of the volume is devoted to an exposition of the subject of statics. In the treatment of statics, much attention is given to geometrical methods and graphical constructions, and this volume is, on the whole, more easily read than the first.

Throughout, the language is clear and precise, and the number of illustrative examples, more especially in the elementary parts, large and well chosen. Answers to the examples are given with here and there a hint for solution. Appended to the various chapters are references to English, French, and German text-books for the convenience of students desiring to pursue farther any matter brought under discussion. This is an excellent feature, but in some cases, as, for example, after "Motion in a twisted curve" (Part I.), or "Constraints" (Part II.), it would have been more to the student's advantage to have given a few examples. In fact, it is to books such as this that we look for illustrations tending to clear up the more difficult parts of the subject.

The most novel part, perhaps, to American readers is the manner of bridging the gulf between kinematics and dynamics. It is done in this way: "The motion of a point moving uniformly from rest in a straight line is determined by the equations

$$v = at,\ s = at^2/2,\ v^2/2 = as, \qquad (1)$$

where s is the distance passed over in the time t, v the velocity, and a the acceleration at the time t.

"If now for the single point we substitute an m-tuple point, *i.e.* if we endow our point with the mass m, and thus make it a particle, the equations (1) must be multiplied by m, and we obtain

$$mv = mat,\ ms = mat^2/2,\ mv^2/2 = mas.$$

"The product mv is called *momentum;* the product ma is called *force;* and the product $mv^2/2$ is called the *kinetic energy* of the particle."

This cuts the knot at once, and will probably be as satisfactory to the student as the labored pages of explanation usually given in connection with Newton's laws of motion. Newton's laws indeed are referred to, but only incidentally, as forming the assumptions on which the application of dynamics to natural phenomena is founded.

Few changes from the usual notation are made. This is a matter for congratulation. One innovation, however, that seems quite uncalled for is the substitution of j for a as the symbol of acceleration. The letter j being appropriated for Joule's Equivalent, it is surely better to retain the old symbol a for acceleration.

A uniform system of notation in mechanics is very desirable, and the recommendations of the International Electrical Congress recently held at Chicago should be followed as closely as possible by writers and teachers. The final report is not yet published, but a preliminary report is given in the *Electrical World* of September 22, 1893.

An important feature of a book on mechanics is that the units that enter be carefully explained. This is done with great clearness in these volumes, but in some cases rather succinctly. All dynamical formulas are expressed in absolute units, and the relations of the British and metric units pointed out. The only slip we have noticed is on p. 7, where the U.S. yard is defined as "the distance between the twenty-seventh and sixty-third divisions of the brass standard kept in the Bureau of Weights and Measures at Washington." This brass bar, by reason of its faulty construction, was never much in favor, and for a long time has been of historical interest only. The Bureau of Weights and Measures regard the U.S. yard as identical with the British yard, or else define the yard in terms of the new International Meter by the relation

$$1 \text{ yard} = 3600/3937 \text{ meter.}$$

The only system of weights and measures that has received the sanction of Congress is the metric system.

The publishers have done their part with great care and taste. Indeed, it is seldom that one sees in a text-book so fine a display of the printer's art.

T. W. WRIGHT.